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EDITED BY

HOWARD C. WARREN, PRINCETON UNIVERSITY

S. W. FERNBERGER, UNIV. OF PENNSYLVANIA (*J. of Exper. Psychol.*)
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THE PSYCHOLOGICAL REVIEW

THE PSYCHOLOGICAL STUDY OF BEHAVIOR [1]

BY WALTER S. HUNTER

Clark University

There has never been a period in the history of psychology in which the experimental contributions to that science have been as numerous as at the present time, or more significant. And yet this productivity is accompanied by an evident uncertainty concerning the subject matter and the fundamental categories of the science. To the extent that psychology has ceased to be the science of consciousness, or immediate experience, it has lost the best criterion by which its field of study could be readily and clearly differentiated from the fields of the other sciences. Formerly even where physiological processes were included in the definition of the science, only those processes were chosen which were thought to be intimately related to consciousness, and the whole event was termed a psychophysical process. With such phenomena for its subject matter psychology could be marked off from the other sciences as definitely as was necessary for any practical purpose. At the present time, however, there is no such obvious method either of giving unity to the field of psychology or of distinguishing between psychology and the related sciences. To be sure no one expects or desires sharp boundaries between the various sciences, but it is right and proper to require that each science justify its separate existence by some significant peculiarity of subject matter or method, however vaguely defined the outer borders of the field may be.

In the present discussion I have tried to make clear some

[1] Address of the president before the American Psychological Association, Toronto meeting, September, 1931.

of the distinguishing characteristics of psychology as a science of behavior. This I have done partly by considering in general terms the handicaps under which psychology has labored, and partly, indeed chiefly, by relating the psychology of behavior to the problems and point of view of physiology. In America we seem to be emerging at last from an era of controversy concerning what psychology is or ought to be. For good or ill the onward march of experiment, which no mere speculation and controversy can halt, has carried psychology along the way of the objective study of human behavior. With the development of institutes for the study of the child and for the investigation of human relations, not to mention the post-war developments in the study of behavior tests and animal behavior, this objective tendency is ever more firmly established. Tonight I am therefore spared the erst-while duty of controversy. I shall not even defend or use the term anthroponomy as a proper designation for the science.[2] The name which the psychological study of behavior is to have must be left in the hands of those social forces which determine such matters. I shall be content if our discussion can serve to clarify some of the systematic problems which confront us.

Our task is not easy, however, because the term psychology covers such a vast field of endeavor. One has only to examine the Psychological Index or the Psychological Abstracts to be impressed immediately by the great range and diversity of the problems under investigation. Indeed it has been said that the great mass of so-called psychological investigations are border-line studies which might just as well be termed physics, philosophy, art, education, sociology, anthropology, medicine, or physiology as psychology, and that the central core of truly psychological work is so small as to approach the vanishing point. Such a situation calls insistently either for the admission that the psychological study of behavior has no distinguishing characteristics or for a serious and sustained effort

[2] W. S. Hunter, General anthroponomy and its systematic problems, *Amer. J. Psychol.*, 1925, 36, 286–302. Also, Human behavior, Chicago, Univ. of Chicago Press, 1928.

to make clear just how such a study differs from those made by the other disciplines which we have mentioned.

In the pursuit of such a task we might attempt to distinguish psychology from the other sciences in terms of methodology. We should thus enumerate such procedures as are involved in discrimination, learning, rating scales, and tests, concluding that wherever such methods are employed there psychology is to be found. By such a route we should, it is true, arrive at an understanding of the great range and applicability of psychological procedures, but our understanding of the nature of the subject matter of the science would be but slightly improved.

I have elsewhere defended the view that psychology can be significantly defined in terms of the specific problems which it investigates. The problems which most clearly separate psychology from the other sciences are those whose investigation has been almost solely the work of psychologists, problems but little if at all studied in the other sciences. Here belong: (1) the study of learning, *i.e.*, the formation and use of habits; (2) the utilization of tests, or sample performances, for the analysis and prediction of adaptive behavior; (3) the analysis of social behavior in terms of interstimulation and response; and possibly (4) the analysis of language behavior. In addition to these problems, which are quite peculiarly the fundamental interests of the psychologist, are others, such as the study of receptor functions and of behavior disorders, in which various other sciences are so deeply interested that the problems cannot be called strictly psychological.

Tonight I propose to attack the problem from another point of view. Granted that there are such general topics as I have indicated to which psychology alone has made any significant contribution, does not the behavior studied by the psychologist possess certain characteristics which serve to set it off from the behavior studied by other sciences? In order to keep the discussion within the necessary time limits, I shall neglect the problem of the relation of psychology to the social sciences and shall concentrate upon its relation to biology. A comparison of psychology and biology can also

serve to indicate certain of the difficulties encountered in the development of our science, difficulties which it is well to recognize specifically in order that efforts may be more adequately made to overcome them.

The psychological study of behavior which to such a great degree, so far as experimental work is concerned, has replaced the psychological study of experience, is the lineal descendant of that older scientific effort. The problems which it attacks and the history which it has are a part of the history and problems of psychology. This is inevitable since the psychological study of behavior has developed within psychology and is not the result of an encroachment of other sciences upon that field. Consequently in order to see clearly what that study of behavior is, we must consider some of the aspects of what the psychological study of experience was. When we have seen some of the reasons why this older effort failed to bring forth important results, we shall be in a better position to appreciate the difficulties that still lie in the path of the study of behavior.

The psychology of experience was unified by the very natural presupposition that the subject matter involved was conscious processes, or immediate experience, with the addition of those physiological processes which appeared to be an integral part of the so-called psychophysical event. This well defined and widely accepted view of the nature of psychology as a whole was accompanied by an almost universal agreement concerning the major subdivisions of its subject matter, or, if we may speak in other terms, an almost universal agreement concerning the subdivisions and constituents of mind. There were, in the first place, those major aspects of mind, the Intellect, the Feelings, and the Will, and in the second place, within these, the minor distinctions of sensation, image, memory, judgment, thought, simple feelings, emotions, and conations. I would not entirely ignore the many dissensions which took place concerning the details of this subdivision of mind, I would only emphasize the essential unanimity with which psychologists held to a classification of mental phenomena not significantly different from that which I have

sketched above. As late as 1911 one writer could say that the content of a textbook in psychology is sufficiently agreed upon to require no comment. How greatly the situation in the science has changed since that time is readily revealed by a glance at present day treatises.

This unanimity of belief which we noted in the older psychology rests upon two fundamental assumptions: one is the philosopher's analysis of the world into mind and matter, and the other is the belief that the constituents and processes of mind are in their very nature knowable by direct observation. The first assumption calls for no comment here, except to note again that it served to determine the limits of the subject matter of the science. The second belief, however, carries implications which are not so immediately evident. If nothing can be in the mind of which the mind is unaware, then no matter how much individuals may disagree concerning details there should be an agreement concerning the main aspects of mental life. Even the man in the street should be able to confirm the scientist's statement that there is thinking, willing, feeling, and remembering. Such famous psychological categories refer so directly and obviously to observable events that it would be remarkable if all men were not in essential agreement on such propositions as the following: Colors can be seen. Tones and noises can be heard. Objects and the movements of objects can be observed. An individual can be joyful, angry, or afraid. He may be pleased or displeased. He can think, judge, and remember. Psychology could apply technical terms to these phenomena. It could go so far as to insist that the phenomena were mental; but it did not discover the phenomena in question. The discovery of seeing, feeling and thinking, if discovery it can be called, was made by common-sense observation at some unrecorded time in the past. The psychologist has made detailed quantitative studies of these processes, and so has brought to light many facts that would otherwise have been unknown; but from the very nature of his point of view he has been cut off, so far as the qualitative aspects of consciousness are concerned, from the possibility of great and important discoveries.

Where experiential psychology has broadened its field to include psychophysical processes, it has met with greater success, although it has failed to discover the mechanisms and factors controlling or accompanying 'mental phenomena.' Significant progress has been made chiefly in the study of receptor processes. Psychology has not solved, to be sure, the nature of the activities which go on in the eye or the ear when vision or hearing take place; but our understanding of these processes is far beyond where the layman could have taken it. On the other hand psychology has had no success in attempting to secure physical correlates for other mental processes, and its search by methods of expression for the essential physical concomitants of affection and emotion has had anything but encouraging results. The most important discoveries in connection with emotion have come not from psychology but from physiology in connection with endocrine and brain functions. The absence of great discoveries in psychology has been pointed out and lamented by Boring [3] in the following words: Psychology "started with such high hopes that all that was needed was the willingness to experiment, patiently, honestly, and industriously, and it has found that mere faith in experimentalism is insufficient for great and rapid progress, unless that faith is accompanied by some flash of insight as to method. . . . Psychology has progressed and developed in a manner similar to the course of any science under ordinary circumstances; but there has not, except for the initial inspiration in the thought of experimentalizing mind, been any great idea or discovery that has revitalized the science . . . opening up new fields, releasing new energy and removing hidden doubts." Boring believes that the absence of important discoveries in psychology is to be explained, in the first place, by the absence of great men (and yet, since men are made great only by their discoveries, the absence of one is merely synonymous with the absence of the other) and, in the second place, by the entanglements with philosophy which psychology has persistently cultivated.

[3] E. G. Boring, A history of experimental psychology, New York, The Century Co., 1929, p. 659.

This latter point is, I think, fundamental, and it is evidenced by the change which has taken place in the subject matter of the science, in the change from investigations of immediate experience to investigations of behavior. *Psychology, unlike the other sciences, has not found it possible to continue with the subject matter bequeathed it by philosophy.*

There is nothing at which to marvel, although there is much to envy, in the enthusiasm of the early days of psychology. Throughout the scientific world those years were a period of important discoveries and growth. The success of the physical and biological sciences naturally led to the belief that mental processes also lay within the reach of scientific analysis and measurement. It seems to me natural that a great confidence in success should have characterized the men who under these conditions first sought to apply the experimental method to the study of mind. Other sciences had broken away from philosophy and the arm-chair tradition to enter a period of immense success. Why might not the science of mind be equally successful if it declared its independence and embarked upon an experimental career? The question was natural; the attempt was inevitable; but the consequences were unforeseen. In the case of the sciences other than psychology, the break with the philosophic tradition has entailed no change in the subject matter of those sciences. Natural philosophy, or physics, continues the study of statics and dynamics. Chemistry is still concerned with the constitution and transformation of matter. Biology continues along the observational lines laid down by Aristotle. Throughout the scientific world the break with philosophy was marked by the increasing emphasis placed upon experiment and not by the repudiation of an old subject matter. The subsequent histories of these diverse sciences have revealed no need for a change in point of view.

We shall make no attempt to determine why this should be so. We shall not consider why it is that such things as stars, rocks, plants, and animals, once studied by philosophers and classified as material objects, can still be studied by scientists without difficulty. This situation may be a tribute

to the point of view of common-sense realism, but we shall regard it merely as a consequence of the practical success in the control of nature which has resulted from the whole hearted application of experiment to this part of the subject matter of philosophy. This success was clearly evident when the first laboratories of psychology were established, and it led to the enthusiastic conviction that experiment could be applied successfully to that other part of the subject matter of philosophy termed mind. Mind seemed to be a part of the world as certainly existent as matter. Indeed its existence seemed even more certain. No experimental psychologist of that time therefore need have feared that the subject matter of his projected science would evaporate or disappear as experimentation developed. The only possibility seemed to be that scientific ingenuity would not be adequate to cope with the intricacies of mind, and this possibility was thoroughly discounted by the spirit of the times.

Psychologists in general are not, I think, dissatisfied with the specific results of the extensive experiments which have been conducted; but in recent years the overthrow of the old categories has left us without the means of introducing a reasonable and fruitful degree of order into the field.

It would seem to me that the primary reason for the failure of *psychophysical* studies to make important progress lies in the unwillingness of the psychologist to study behavior for its own sake. He has been content to pick and choose from the forms of behavior discovered by the physiologist those tems which held out most promise of being related to conscious processes. The result has been, on the one hand, that the psychologists have not equipped themselves for the discovery of intraorganic behavior, and, on the other hand, they have contented themselves with what any student of behavior would call a haphazard and piece-meal selection of physiological data. However, if a given science is to study *psychophysical* processes, it cannot feel free, at least such a science has not felt free, to study any one physical process in its entirety. The investigator studies the relation of emotion to respiratory changes without feeling forced to understand the process of respiration

in its entirety. Or he chooses oxygen consumption, tremors, changes in blood pressure and in the electrical resistance of the skin, selecting here and there from the physiological processes without ever forcing himself to master the details of the interrelations of these phenomena.

Not all of the forms of behavior studied by the old psychology have been of this physiological, or subcutaneous, type. At an early date opinions, observations, and experiments were offered on such forms of behavior as habits, reflexes, and instincts, and with time the list has been greatly extended to include such topics as intelligent behavior, genius, personality, sex, language, and learning. Such forms of behavior are superficial rather than subcutaneous, or intraorganic, and they have been studied for the most part without any reference to possible accompanying conscious states. This behavior, except for reflexes, has not been studied by the physiologist. It involves essentially the adaptation of the organism to the environment, and it has been termed the behavior of the organism as a whole, a characterization which we shall later comment upon adversely. I would not ignore the fact that the biologist has made important contributions to the problem of behavior adaptation nor that the sociologist and the anthropologist have also had something to add. Nevertheless, the psychological student of behavior has here an important and unique rôle to play.

It has been customary, in tracing the historical influences which have molded our science, to emphasize the important rôle played by physiology in the development of psychology and to neglect, relatively, the great importance for this growth of those modes of thought which derive particularly from the great naturalists of the Darwinian period. And yet these latter men have influenced more fields of psychology than have the physiologists. We have just seen that physiological material was utilized by earlier psychologists primarily in connection with problems bearing upon 'conscious' processes. Experimental psychology, as the term is currently used, had its origin in the labors of the psychologist and the physiologist on the problems of the senses. This psychology has always

followed closely the physiological tradition without ever becoming frankly physiology. The remainder of psychology (animal and child behavior, tests, business and educational psychology, and even the field of behavior disorders) has been less physiology than it has been the natural history of man. In this natural history, or ecology, attention is focussed upon the manner in which man adjusts himself to the problems and crises which the environment presents and not upon a correlation of mental and physical processes.

The study of human adaptability came into psychology first through the behavior studies of such men as Darwin, Romanes, and Lloyd Morgan, and later, in America, through the influence of the functional psychologists, the students of animal behavior, and the behavior testers. This method of approach to the study of man is, on the one hand, markedly different from that followed by the philosophers, who, from Locke to Mill, had been analyzing mind into its constituent parts. It is also to be contrasted, on the other hand, with the point of view of the physiologists who are more interested in the study of that subcutaneous behavior which is intrinsic to the various anatomical structures of the body than they are in the study of the success or failure which marks the individual's adjustment to the environment and which constitutes such an important aspect of the organism's life history.

Psychology cannot claim the whole problem of the adaptability of biological processes. This is much too large a topic, involving as it does the whole kingdom of living organisms as well as all activities of these organisms. Psychology's primary concern is with man; and in man its chief contemporary interest lies in those forms of behavior which are externally observable and externally adaptive, behavior which might legitimately be termed superficial as opposed to subcutaneous. The description and explanation of the subcutaneous processes is peculiarly the task undertaken by physiology. Thus physiologists deal typically with digestion, circulation, brain functions, and glandular secretions, while psychologists deal characteristically with such behavior as manual dexterity, anger, and habit formation in the superficial

parts of the body. Even in the study of personality where at first thought it might be said that the subject matter was subcutaneous responses, the psychologists are directly concerned only with superficial, *i.e.*, externally observable, behavior. Introvert, extrovert, paranoid, and hysterical traits are identified in terms of the adjustments made to the external environment. It is these adjustments which furnish the basis for the criteria of normality and abnormality, for no criteria can be derived from intraorganic sources, although one may postulate a variety of such factors in explanation of superficial behavior.

The relative neglect by physiology of behavior which is externally adaptive and the relatively great emphasis which psychology places upon this problem is eloquently testified to by the psychologists' reception of such work as that of Cannon on emotional behavior. This work has attracted the attention of psychologists not merely because it has been related to the James-Lange theory of emotion and so to an ancient category of psychology, but because Cannon himself has stressed the adaptive value of the behavior in the organism's struggle for existence. It is not to be expected of course that all psychological studies will contain interpretative comments upon adaptation, but in the selection of the behavior to be studied, the external adaptive value of the response will have an important rôle.

When physiology undertakes the study of a given type of behavior, let us say digestion, it seeks to examine that process in all of its details with reference to what it is, how it is initiated, and what other forms of behavior interact with it. In carrying forward such a program the physiologist is greatly aided by the fact that 'digestive activities' are a well established category of behavior in his science and by the fact that specific instances of digestion can readily be recognized as typical of that class of behavior. The investigator is thus free to concentrate his efforts upon the perfection of his experimental procedures. In psychology we are carrying forward a similar program, but we suffer greatly from handicaps which are due partly to the youthfulness of the behavioristic move-

ment and partly to the difficulty of bringing human adaptive behavior into the laboratory or at least under analytical control. The youthfulness which I have mentioned is clearly indicated by the absence of a well established classification of behavior which sooner or later will take the place formerly occupied by the categories of a psychology of experience. In the present-day categories of behavior there is a curious mixture of old and new as is indicated in such terms as thinking, emotion, discrimination, learning, voluntary action, personality, and intelligence. The student of behavior, as his critics have said, has in many instances merely taken over a mentalistic category and given it an objective definition.

As a student of the phylogenetic aspects of behavior, I may be pardoned if I point to the work on the maze habit as an indication of what I think needs to be done in the analysis of each of those immensely diverse forms of behavior which make up the subject matter of psychology. In the first place the category of learning, or habit formation, is well established in psychology, and the mastery of the maze is a clear-cut instance of that behavior. In contrast with the concept of learning those of emotion, affection and personality, even as used by students of behavior, are vague and confusing, with the result that it is difficult to secure bona fide instances of the behavior for study. In the second place, the maze habit has been examined from the point of view of methodology (including methods of training, criteria of mastery, and reliability), from the point of view of sensory-neural control, and from the point of view of the influence of such factors as age, sex, species, drugs, incentives, and intelligence level. Out of this wealth of experimentation has come a much clearer conception of just what maze behavior is as well as a better understanding of the effects of the above conditions upon one class of externally adaptive behavior. As in the case of the maze, all psychological studies are concerned with specific instances of behavior whether one is dealing with play, personality, or motivation, and the careful analysis of this behavior should not be too long postponed if an adequate basis for generalization is to be found.

So far in the discussion I have sought to sketch some of the reasons why the psychology of experience failed to bring forth important results, and I have sought to indicate that the study of behavior gradually grew up under the joint influence of this psychology and of biology, particularly the biology of the Darwinian tradition. It remains for us to compare contemporary psychological and physiological studies of behavior in order that we may see as clearly as possible just what the distinctive subject matter of psychology is.

(1) Psychology stresses externally observable behavior, whereas physiology studies internal, subcutaneous activities.
(2) It is sometimes said that psychology studies both consciousness and behavior, whereas physiology studies only behavior.
(3) Psychology, except in the case of receptor processes, emphasizes functions that are extrinsic to the various structures of the body, while physiology specializes in intrinsic functions.
(4) Psychology stresses adaptation to the social environment, whereas physiology ignores the social aspects of the environment even where adaptation to the external environment is considered.

Of these points of contrast between psychology and physiology, the first one has already been sufficiently examined for our present purpose. When current psychology concerns itself with subcutaneous factors in behavior it seldom does so except on the basis of inference. Such factors as age, intelligence level, and the effects of drugs may be invoked to account for experimental results, or explanatory data may be borrowed directly from physiology, but the psychologist shows a marked aversion to determining directly and experimentally what goes on under the skin of the organism. The attitude of the physiologist is just the reverse. It is almost as though the physiologist had said that he would not study the success or failure of the organism's responses because the skin is the boundary of the organism and such a program would extend beyond these limits. The actual reasons why the physiologist

has so definitely avoided the behavior studied by the psychologist are quite other. Perhaps the three following are the most important: (1) the tradition of anatomy; (2) the belief that mind was involved in psychological behavior; and (3) the difficulty of the experimental control of such responses. Of these the belief in mind is the only serious reason why physiologists should longer avoid the field. And to the extent that the behavioristic program in psychology succeeds we may expect to see physiologists studying what are now known as the psychological aspects or forms of behavior.

The second point of contrast between the two fields is the traditional contention that psychology studies consciousness as well as behavior whereas physiology studies only behavior. Concerning the status of the latter science there can be no reasonable doubt. It is true that many physiologists, if questioned, would maintain that they used consciousness in their work, although never making it an object of study. Such opinions are derived, however, from extraneous reading in philosophy and experiential psychology rather than from physiology, and so they carry little scientific weight when coming from the physiologist. As we said earlier, the study of consciousness has greatly decreased in America although elsewhere, particularly where psychology is still united with philosophy and where research is directed along the traditional lines of experimental psychology, consciousness is still very much in evidence. We need not concern ourselves here with the question whether or not, in America, consciousness is merely ignored, or entirely denied. The net result for science is the same.

There are so few studies made of imagination, memory, emotion, and thinking, considered as conscious processes, and relatively so many studies made from this point of view of sensation and perception that I believe in America the chief experimental support for the belief in consciousness comes from this latter field. Experimental work can be so excellently done in such domains as vision, hearing, and cutaneous sensitivity that some psychologists believe not only that consciousness exists and can be scientifically studied in the

form of sensation and perception, but that it exists and can be the subject matter for science in the other forms made classical by psychology. In order to avoid controversy so far as possible I shall content myself here with comments on but a single aspect of the problem—that concerning the misdirection of interpretation and effort in the field of sensory processes. Let us use the psychological study of music as an example, since thereby we can also illuminate another difference between physiology and psychology, *i.e.*, their difference in attitude toward the social environment, a topic to which we shall return later. Everyone admits the existence of tones, noises, melodies, and harmonies. There is no evidence, however, that these are mental, and hence there is no obvious justification for their investigation by the student of human nature. The world is full of auditory phenomena as it is of plants, rocks, and clouds. Only if these phenomena are mental does the experiential psychologist have a reason for examining their intrinsic character. And yet even a brief reference to the classical works on audition and music will reveal a great mass of accumulated information on the intrinsic characteristics of the auditory components of the world, information whose gathering is but poorly justified by the statement that some one should secure the data, and, since other scientists are not inclined to do so, the psychologists should take over the task. The psychological study of audition, as I see it, has but three real warrants, of which the first two are applicable also to physiology: (1) to afford a basis for an adequate theory of the functioning of the ear; (2) to furnish data on the organism's capacity for emotional and non-emotional responses to auditory stimuli; and (3) to correlate one of the most important *products* of man's activity, music, with the individual's capacity to create these products. This third point in particular is greatly in need of interpretative elaboration, an elaboration which I have not the time, and, I fear, not the ability to make adequately. Music is not merely a phenomenon in the world; it is also a product of man's activity. In this respect it is in a class with painting, sculpture, tools, and houses. The study of such phenomena

has psychological justification, but only if it is so directed as to throw significant light upon the nature of man. It is, however, my belief that the study of music has been directed toward the history and analysis of music itself rather than toward an analysis of the fundamental characteristics necessary in man to produce and to respond to music. However, much of the work of Seashore and his students is a beginning along this latter line of investigation.

With reference, then, to the field of sensation and perception as a whole, I should say that what the behaviorists are doing involves less a change in actual methods of experimentation than a change in interpretation and emphasis. Let it once be clearly recognized, as I think in America we are doing, that the mentalistic hypothesis is unnecessary even in the field of receptor processes, and the last significant stronghold of consciousness in experimental psychology will be gone. No experimentalist will then attempt to differentiate psychology and physiology on the basis of consciousness.

Let us now examine the third distinction between psychology and physiology, which was formulated as follows: Psychology, except in the case of receptor processes, emphasizes extrinsic functions, while physiology specializes in the study of functions which are intrinsic to the various organic structures. This statement is to be contrasted with the one which is current, and which I myself have at times supported, to the effect that psychology studies the behavior of the organism as a whole whereas physiology studies the behavior of parts of the organism. One cannot study respiration without relating that function to many of the other activities of the organism. The consideration of digestion involves the study of many widespread bodily activities. Organic functions are so thoroughly integrated that there is ample reason for saying that the physiologist deals with the organism as a whole, while, on the other hand, many of the forms of behavior studied by psychologists involve no obvious total activity of the organism. Such behavior as reading, writing, tapping, card-sorting, and muscular work involve predominantly localized activities which are quite comparable, from the standpoint of

a part vs. the whole of the organism, to the functions studied by the physiologist. The significant distinction between the two sciences involves the characteristic of intrinsic vs. extrinsic activities rather than that of part vs. whole behavior.

Physiology, we have said, emphasizes the functions which are intrinsic to the various structures of the body. In this way it studies: vision, the function of the eye; respiration, the function of the lungs; secretion, the function of the glands; contraction, the function of the muscles; and conduction and integration, the function of the nervous system. Wherever there are organic structures, physiology examines their functions and the interrelations of these functions. This science is therefore never in doubt concerning its fundamental and proper subject matter, nor does it encounter serious difficulty in classifying the phenomena studied, because these phenomena can be rather simply and clearly grouped as functions, or interrelations of functions, of the structures concerned. The contrast with psychology is very marked except in the above noted case of the receptors. Where psychologists study vision, hearing, taste, and smell, they are dealing with functions intrinsic to certain structures just as truly as are the physiologists; and it is to be noted that this very structure-function relationship makes it easy for the psychologist to classify his data on receptor processes, *i.e.*, to group his material as visual, auditory, or olfactory. In contrast with this situation let us consider another which concerns the functions of the hands and feet. The functions intrinsic to these structures are locomotion and prehension, and physiology has something to say concerning these topics. Psychology, however, has but little interest in such functions or in the various reflexes of the arms and legs, whereas it has an immense interest in such activities as writing, typewriting, pointing, shooting, gesticulating, maze-running, and playing the piano. In all of these activities a large amount of habit formation is involved; but the presence or absence of learning is not the basis for the distinction between what we might call psychological and physiological functions, for conditioning plays a large and often undetermined rôle in the behavior

studied by physiology. Rather the psychological functions are: (1) types of behavior which may be added almost indefinitely to the intrinsic action repertoire of a given organ such as the hand; and (2) types of behavior which to a large extent are dependent neither upon any single organ for their manifestation nor upon any fixed mode of response of any organ. Thus the same language behavior may be executed in French, German or English by the hand or by the vocal organs, and in music the same melodies may be sung in different keys. Here we have equivalent types of behavior[4] but no identity of responses. Furthermore it is to be noted that such behavior as language, tapping, and maze solution may at one time involve responses of the right hand while at another time other parts of the body may exercise these functions. There is, so to speak, very little vicarious functioning between the various organs studied by the physiologist, but a very great amount between the various organs involved in the studies of the psychologist. The functions of the nervous system, the lungs, the muscles, the liver, the kidneys, and the heart are so highly specific to these organs that no other structure can take over the function. In the activities studied by the psychologist there is on the other hand a large amount of vicarious functioning of one part of the body for another. If one hand is lost, writing can be carried out by the other hand. If both hands are lost, the feet can take over the function. If hands and legs are all gone, the mouth parts can function in holding the pencil. Writing is not a form of behavior intrinsic to a given body structure. Language responses can and do shift from one part of the body to another as circumstances demand. The same is true of maze-running. Problem solution of the type called thinking may be normally executed with the vocal mechanism, but in the event that this latter is lost the function is performed elsewhere. *Typically, psychology deals with behavior which although normally performed by certain bodily structures in certain ways need not be so performed.*

In contrast with the phenomena just described the func-

[4] For a discussion of this topic see my Human behavior, Chicago, Univ. of Chicago Press, 1928, pp. 318–325, and A. P. Weiss, A theoretical basis of human behavior, Columbus, O., Adams, 1925, p. 84.

tions of the special senses in relation to their proper stimuli are highly intrinsic and admit of little or no vicarious functioning. The study of such processes is therefore not the type of enterprise which would distinguish psychology and physiology, and we should not be surprised to find both sciences for different reasons busy with the project. Sensory functions are not determined by a direct examination of the receptors but indirectly through the behavior aroused. To the extent that this behavior is intrinsic to a specific organ, as for example the electrical changes in the eye, psychology has less concern with that behavior than where the receptor functions are revealed by extrinsic responses. Thus, to continue with vision, the functions of the eye can be inferred from the study of variations in the behavior of the hand which indicate whether or not a given discrimination has been made. Since the same inferences can be drawn from vocal behavior or from the activity of running through a discrimination-box, we say that the discriminative behavior is extrinsic to any structure. Physiology also will utilize such types of behavior to throw light on the specific functions of a receptor, but only psychology will then turn about and study the extrinsic behavior which served as a medium for this determination.

What shall we say concerning the explanation of this difference between the intrinsic functions which are characteristically studied by the physiologist and the extrinsic functions which are so typical of psychology? We cannot say simply that psychology studies those forms of behavior which are controlled by the central nervous system and physiology those forms of behavior which are controlled chemically or by the autonomic system. The interactions of these three systems of control are much too intimate[5] for any such hypothesis to be valid. We cannot even be certain that psychology is typically concerned with behavior determined by the cerebrum or by the cerebral cortex. Our knowledge of neural processes (and of behavior) is as yet much too inadequate for any such procedure. Psychology chooses certain forms of behavior

[5] See W. B. Cannon, The autonomic nervous system: an interpretation, *Lancet*, 1930, 218, 1109–1115.

for study on the basis of characteristics which rightly or wrongly it finds in that behavior itself. Sometime in the future it may be possible to say that these forms of behavior have typically a certain kind of neural control; but even then, since neural control is not open to direct observation, the actual selection of behavior for study must be made upon some other basis. At the present time we may reasonably assume that the forms of behavior which constitute the *core* of the subject matter of psychology are more closely related to cerebral activity than to other neural processes and that the so-called vicarious functioning to which we have referred is partly due to this cerebral control and partly to the fact that the behavior concerned is to such a large degree an activity of skeletal muscles.

In any case we must not confuse the vicarious functioning which we have described in behavior with the vicarious functioning of the nervous system where one or more neural centers may take over the function of areas that have been destroyed. Vicarious functioning of the nervous system is not involved in any recognized way in those cases where a right-handed individual becomes perforce left-handed after the loss of his right hand. After a process of re-education the left hand functions for the right because the behavior concerned is not of the specific kind which only the right hand can execute. As opposed to this, salivary, gastric, and adrenal secretions can only be made by the specific glands in question. Digestion requires the digestive system as well as the specific neural centers and chemical activities which control it. In the absence of the digestive system no amount of re-education can reestablish the process. Within a given physiological system one finds a considerable amount of compensatory adjustment where the functions of defective elements are partly supplied by those that are normal. Organic functions are so closely interrelated that any change in one will call forth adjustments in others even across the boundaries of physiological systems. In the functions studied by psychology this is but slightly if at all the case. Deficiencies in maze-running, tapping, truth-telling, mathematical performance, or anger do not seem to

entail compensations in other forms of behavior. The relationships between the responses studied by psychology are apparently not of this type. Perhaps compensations occur in the form pointed out by the students of personality, but the reasoning which supports this view is still on a basis of analogy, and in any case it serves to give point to our argument rather than otherwise. *The organism in its subcutaneous adaptations is much more highly integrated than it is in its supercutaneous adaptations, that is to say, there is a much closer interdependence of behavior inside than outside of the organism.* Along with this fact goes a very great uniformity in the intra-organic situations which must be met by the organism as opposed to a very great heterogeneity in the external situations.

The distinction between physiology and psychology on the basis of the intrinsic or extrinsic character of the behavior studied has very important consequences for the classification of behavior in the two sciences and so for the clarity with which the subject matters of the two fields can be organized. As we pointed out above, physiology encounters no particular difficulty in classifying behavior because that science can usually if not always group responses in terms of the structures concerned. Psychology has no such easy road to follow, except in the case of the receptor processes and a few ultra-simple responses which it currently investigates. How are we to classify the bewildering multiplicity of responses which the psychologist studies? How are we to indicate that certain responses belong to one class and other responses to another class? Typewriting, singing, swimming, writing, talking, maze-running, puzzle solving, cab driving, clerical work, anger, prayer, and sex behavior are activities which the psychologist should be able to group in some significant and helpful way. I cannot attempt an answer to these questions in the present context. I must be content to emphasize the existence of the problem. Consciousness psychology reached a fairly definite solution of its problem of classification, a solution largely taken over from the philosophers. All consciousness was divided into the Feelings, the Intellect, and the Will, and these

were again subdivided into the categories which are so well-known. Until recently all of the treatises testified to a general satisfaction with this situation; but now even confirmed believers in consciousness can raise such questions as 'Is emotion merely a chapter heading?'

Writers on the psychology of behavior have as yet developed no widely accepted mode of classification. The general division of behavior into instinct and habit, where this is accepted, is based on the genetic history of the behavior concerned, but the identification of specific habits and instincts is made on a variety of bases. Dunlap long ago called attention to the difficulty of identifying instincts on the basis of the actual behavior concerned; and an examination of Watson's descriptions of fear, anger, and love behavior in infants will reveal that the distinctions are but slightly justified by the actual behavior observed. Or again, if one sees a dog running, no mere observation of that behavior can determine whether the activity is play, sex behavior, anger, or hunting. The identification of the behavior studied by the psychologist is made at present largely in terms other than the bodily structures involved or the specific responses made. Maze-running and typewriting are identified in terms of the apparatus employed; nest-building and food-getting are identified in terms of the result of the behavior; while different types of emotional responses are identified largely in terms of the situations which call them forth. It is doubtful whether any one writer can propose a system of classification which will bring adequate order into the field. Rather we must wait for the results of trial and error efforts on the part of many investigators before we can give a reasonably satisfactory answer to the problem of the classification of psychological forms of behavior.

We come now to a brief consideration of the fourth and last distinction which we have made between physiology and psychology, that, where physiology considers the organism's adaptation to the external environment, no account is taken of the social aspects of this milieu, whereas the consideration of this social phase of the stimulus and response is one of the

outstanding characteristics of psychological work. I do not refer here merely to the investigations of the social psychologist and the student of racial differences in behavior. I have in mind, as Weiss had when he proposed the term *bisocial*, nearly if not quite the whole of psychological work. Physiologists may study the temperature regulation of the body in relation to external temperatures, or they may study the maintenance of bodily equilibrium; but it is the psychologist who chooses for experimental study such relations between the organism and the external environment as the following:

(1) Behavior aroused by stimulations in which other persons are integral parts, as in jealousy, anger, rivalry as an incentive for work, or ascendancy-submission;
(2) Behavior whose results are socially important, as in such diverse activities as typewriting, home-building, personnel selection, and mathematical behavior;
(3) Behavior which serves as a tool for social conduct, as in the language activities and the behavior involved in the mastery of school subjects; and
(4) Behavior maladjustments which are shot through and through with relationships to the individual's social environment.

This emphasis upon the behavior which is related to the social life of the group seems to me to result quite naturally from the psychologist's interest in the organism's adaptive behavior to the external environment and from his interest in extrinsic behavior. In relation to the external environment nothing is more significant for survival than the behavior which is related more or less intimately to the social group. With such a subject matter qualitative analysis and description are relatively easy. A significant quantitative analysis is a much more difficult and elusive goal, while the discovery of the fundamental subcutaneous mechanisms which are involved in the control of such behavior is the most difficult task of all.

In attempting to indicate the essential characteristics of that behavior which constitutes the core of the subject matter of psychology, I have found it necessary to use such terms as

external adaptation, extrinsic behavior, and social environment. If I were now to attempt a definition of contemporary American psychology, not as it ought to be, nor as I wish it were, but as I think it is, the result would be as follows: *Psychology seeks to describe and explain, to predict and control, the extrinsic behavior of the organism to an external environment which is predominantly social.* It is thus to be contrasted with physiology which describes and explains that behavior which is intrinsic to the various structures of the organism, behavior which is essentially subcutaneous and which involves adaptations to the highly uniform internal environment.

Will some historian fifty years hence write that the psychological study of behavior has brought forth no important discoveries? It is too early by fifty years to answer this question! However, it would certainly seem that no new types of psychological behavior will be discovered. The layman and the shrewd observer of human nature long ago discovered such phenomena as habit, learning, personality, aptitude, motivation, prayer, thinking, and custom. We must look for psychological discoveries in the realm of the factors which control behavior and in the field of the interrelations of behavior. It is to be hoped that psychologists will overcome their aversion to delving into the field of subcutaneous behavior so that they may share with physiologists the duty and the honor of revealing some of the as yet unknown inner processes which vitally affect the extrinsic behavior of the organism. Such an active liaison between the sciences of psychology and physiology could not but promote the best interests of each and bring us measurably nearer our goal, the understanding and control of human conduct.

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THE GOAL GRADIENT HYPOTHESIS AND MAZE LEARNING

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One of the most persistently baffling problems which confronts modern psychologists is the finding of an adequate explanation of the phenomena of maze learning. The writers in this field a generation ago seem to have regarded it as a relatively simple process. This view is rapidly losing ground. It is now quite generally agreed by those familiar with the experimental literature on the subject that the process is in reality an exceedingly complex one, that it differs in important respects from the numerous other forms of trial-and-error learning, and that no mere chain-reaction hypothesis alone is adequate to account for the various known facts. The revolt against the over-simple view continues, nevertheless, to motivate a great deal of experimental work. This has, for the most part, been conducted with animals, usually rats. Upon the whole, this work has been admirably conceived and the results have a significance far beyond the negative one of disproving the efficacy of the law of frequency as a sole explanatory principle. It is noteworthy, however, that writers in this field have been conspicuously more successful in disproving the explanatory potency of hypotheses of the early Watsonian type than in devising more adequate formulæ. Meanwhile the steady accumulation of well authenticated maze-learning phenomena, all demanding explanation by any thoroughgoing explanatory hypothesis, is making the task constantly more insistent and at the same time more complex and difficult. These problems present two major aspects, each dependent upon a fairly distinct mechanism. It is believed that the *modus operandi* of the two mechanisms is sufficiently distinct to make separate treatment desirable.

The mechanism which in the present paper will be mainly depended upon as an explanatory and integrating principle is

that the goal reaction gets conditioned the most strongly to the stimuli preceding it, and the other reactions of the behavior sequence get conditioned to their stimuli progressively weaker as they are more remote (in time or space) from the goal reaction. This principle is clearly that of a gradient, and the gradient is evidently somehow related to the goal.[1] We shall accordingly call it the *goal gradient hypothesis.*

I

One of the simplest and at the same time most illuminating experiments in maze learning is one which presents the animal with two alternative paths from the starting point to the food box. At first the rat will take one or the other path, more or less at random. In case one of these paths is distinctly longer than the other, however, the animal will gradually come to choose the shorter path.[2]

In order to consider the theoretical aspects of the situation, let it be supposed that a hungry albino rat has been released at (*S*) in the maze shown in Fig. 1. He has found his way to

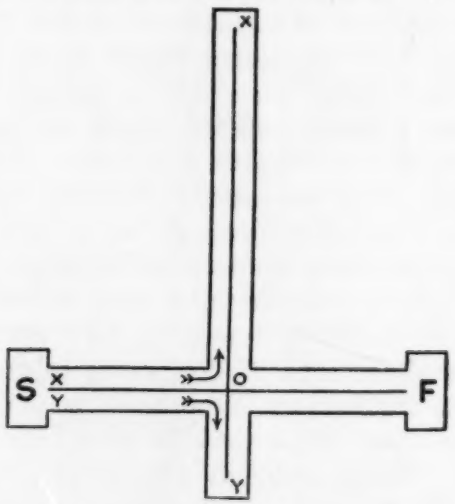

FIG. 1.

[1] This principle has been used to advantage by Washburn in the development of the theory of learning presented in the third edition of The animal mind (1926). See especially p. 329 ff.

[2] J. E. DeCamp, Relative distance as a factor in the white rat's selection of a path, *Psychobiology*, 1920, 2, p. 245.

the food (F) an equal number of times by the long path (X) and the short path (Y). For convenience we shall divide these paths up into sections of equal length, the X-path totaling six units in length, and the Y-path, three.

The stimulus-response dynamics of this situation at a fairly advanced stage of practice is conceived to be as represented in Fig. 2 for paths X and Y separately. The upper

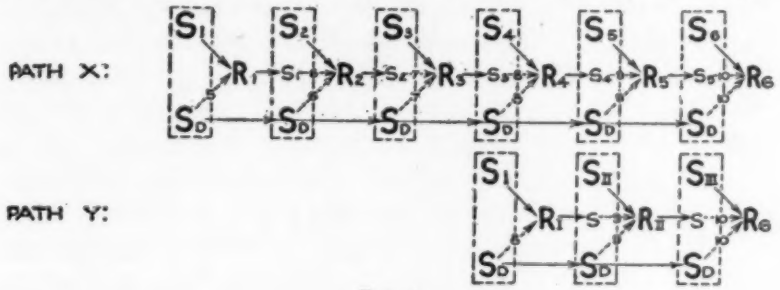

FIG. 2.

series of S's in each sequence represents the external stimulation. The R's represent the responses by the organism. The s's represent proprioceptive stimulations resulting from these movements and the S_D's represent the persisting or drive stimulus. The dotted rectangles represent redintegrative stimulus complexes. It will particularly be noted that the respective sequences, in addition to possessing a common drive stimulus, are also alike in possessing a common beginning and a common ending; the former, however, being a *stimulus* and the latter a *reaction*.

The dotted arrows extending from the s's and the S_D's indicate excitatory tendencies acquired through the process of association or conditioning. At this point enters the principle of the goal gradient according to which these acquired tendencies leading to the goal reaction are strongest, and are progressively weaker to reactions more and more remote from the goal. In this connection a very simple gradient has been assumed—that the excitatory tendency to the R_G at some advanced stage of training is ten units, and that at each step removed from the goal the strength of the excitatory tendency is diminished by one unit. These values have been placed

on the lines representing the excitatory tendencies in Fig. 2. Presumably this gradient is operating on all the acquired excitatory tendencies. In the interest of simplicity of exposition, however, only those values emanating from the drive stimulus will be discussed in the following pages.

From an inspection of this figure it is easy to see that the choice of the shorter of the two paths must follow. The stimulus leading to the alternative responses at the outset being the same, the issue as to which path will be chosen must depend upon the relative strengths of the divergent excitatory tendencies, the one to the first act of the X path (R_1), and the other to the first act of the Y path (R_I). The strength of the tendency to the former is 5 units, whereas that to the latter is 8 units. Evidently R_I must take place rather than R_1. The principle of the goal gradient thus clearly generates the preferential choice of the shorter of two alternate paths to a goal. In this important sense the former may be said to explain the latter.

II

In the preceding discussion of the choice of the shorter path, as well as in two previously published studies involving rather distinct aspects of adaptive behavior, the concept of the goal gradient has been utilized in its simplest possible form, *i.e.*, as a uniform slope. This type of gradient is shown diagrammatically in Fig. 3. Because of its simplicity it is

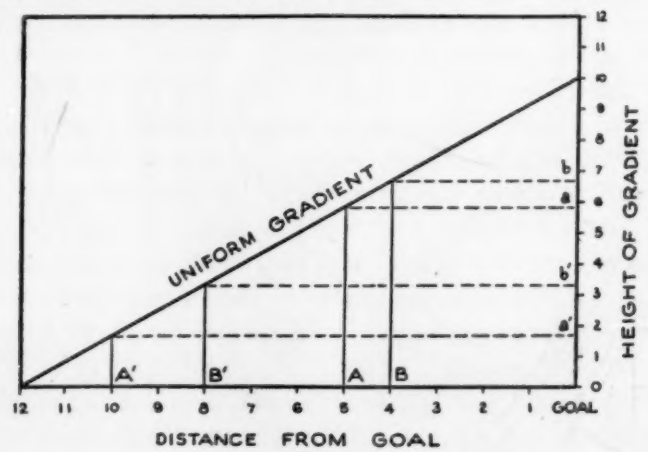

FIG. 3.

admirably adapted to the needs of exposition. Moreover, where only very elementary results were to be deduced from the principle, such as the short-circuiting of pure-stimulus act sequences,[3] the evolution of goal attraction motor mechanisms [4] and the choice of the shorter of alternative paths in the maze, an excitation gradient sloping generally upward in the direction of the goal was all that the situation required. But with the attempt to utilize the principle for the explanation of the finer and more exact details of adaptive behavior there comes the necessity of inquiring rather specifically into the mathematical characteristics of this slope. The method to be followed will be to determine which of various conceivable slopes is required to generate the phenomena revealed by one of the best, and, from the present point of view, one of the most exacting of recent maze investigations.[5] Having determined the nature of the gradient in this manner, various behavioral phenomena of maze learning will be deduced from it, some of them known from experiments already performed, some of them not as yet submitted to experiment. In the former case the hypothesis will appear in the rôle of an explanatory principle in the most significant meaning of the term. In the latter it will serve as the basis for prediction.

Yoshioka's excellent experiment furnishes evidence peculiarly adapted to give us at least a first approximation to the curve of the goal gradient. He ran rats on two mazes of the alternate-pathway type represented by Fig. 1. One maze was exactly twice as large as the other as regards the length of the pathways, the standard path in the smaller measuring 211 inches in length, whereas the corresponding path of the larger maze measured 422 inches. The second alley of each maze was so constructed that it could be shortened at will. The problem was to determine the ease of discrimination by the rats of various absolute and relative amounts of difference

[3] C. L. Hull, Knowledge and purpose as habit mechanisms, PSYCHOL. REV., 1930, 37, 511–525.

[4] C. L. Hull, Goal attraction and directing ideas conceived as habit phenomena, PSYCHOL. REV., 1931, 38, 487–506.

[5] J. G. Yoshioka, Weber's law in the discrimination of maze distance by the white rat, *Univ. of Calif. Publications in Psychol.*, 1929, 4, 155–184.

between the alternate pathways. Groups of 20 rats were run on each of five different settings of each of the two mazes, nearly 200 animals in all being used. The *relative* length of the long alternative to the short one at each of the five settings was the same for both mazes, the ratios being: 1.07, 1.14, 1.23, 1.33, and 1.44. The *absolute* differences between alternate paths were, of course, twice as great in the large as the small maze. Within the range explored by Yoshioka the results show a very definite tendency for the discrimination of equal ratios to be equally difficult, regardless of the absolute amount of difference between the lengths of the paths being compared. Thus the small maze, with a difference between alternative pathways of 65 inches, gives almost exactly the same mean discrimination score as the long maze with a difference between its paths of 130 inches.

The implication of these results as to the shape of the goal gradient may be best appreciated by first considering what results might be expected from an experiment of the Yoshioka type, with various assumed gradients. A preliminary analysis of the shapes a goal gradient might conceivably take reveals three typical ones. The first is the uniform slope represented by the straight line and already discussed. The second is a curve of negative acceleration, the increase in the excitatory tendencies being more rapid at the beginning of the sequence and growing less and less as the goal is approached. The third is a curve of positive acceleration in which the increase in the excitatory tendency is relatively slight at the outset of the behavior sequence, but grows progressively greater as the goal is approached.

The situation as regards the uniform gradient is represented in Fig. 3. Let us suppose that the standard path in the small maze is 5 units long, and in the large one it is 10 units long. Suppose, further, that the alternative path in the long maze is 8 units long, and in the short one it is 4 units long. The difference in length between the alternative paths of the large maze is two units, and between those of the small one it is one unit. Turning to Fig. 3 it will be seen at once that the difference between the stimulus gradients (projected by means of

TABLE I

	Small maze	Large maze
Standard path	5 units long	10 units long
Comparison path	4 units long	8 units long
Difference in excitation gradient	1 unit	2 units

the broken lines upon the scale at the right) in the first case is twice as great as in the second. Since it is assumed that the degree of the preference for the shorter path is based upon the absolute difference in the excitatory gradient between the two alternatives, a straight-line goal gradient would produce a much more ready discrimination in the long maze than in the short one. This, we have seen, is contrary to Yoshioka's results. These demand equal discrimination in our suppositional situation, since the differences in distance are proportional (1/5) in both cases. Clearly the evidence does not point to a goal gradient of uniform slope.

We may inquire, secondly, what might be expected if the gradient were one of negative acceleration, such as that shown in Fig. 4. As before, we measure off on the base line values of

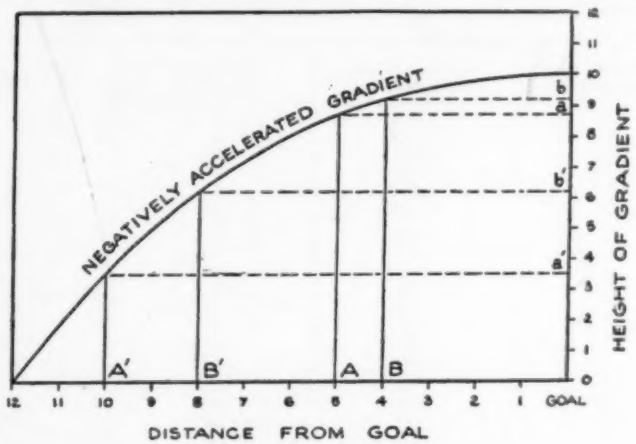

FIG. 4.

5 and 4 units for the short maze, and of 10 and 8 units for the long maze, and then erect a perpendicular from each point so as to locate the excitatory tendency to the choice of each at the entrance to the respective pairs of paths. We then project these gradient values by means of broken lines upon

the scale at the right. Inspection of the figure shows that the gradient difference in favor of discrimination of the long pair is about three times as great as for the short pair. This is in even more violent disagreement with Yoshioka's results than the corresponding implications from assuming the uniform gradient of Fig. 3.

By a process of elimination we thus arrive at the tentative conclusion that the goal gradient must have a positive acceleration. There remains, however, the question of determining the exact nature of this type of acceleration. Mathematicians have devised and studied a great variety of plane curves and curved figures, certain portions of which satisfy equally well the rather inexact requirement of manifesting positive acceleration. There are, for example, the circle, the elipse, the

TABLE 2

This table shows the strength of the excitatory tendency of the drive stimulus to evoke reactions habitually taking place at different unit distances from the goal reaction, on the hypothesis that the goal reaction has a strength of ten points and that the strength of the excitatory tendency to the remaining reactions diminishes progressively with remoteness from the goal according to the logarithmic principle.

Units distant from goal	Strength of excitatory tendency	Units distant from goal	Strength of excitatory tendency
1	10.000	26	1.903
2	8.277	27	1.809
3	7.270	28	1.718
4	6.554	29	1.631
5	6.000	30	1.547
6	5.547	31	1.465
7	5.164	32	1.386
8	4.831	33	1.310
9	4.539	34	1.236
10	4.277	35	1.164
11	4.040	36	1.094
12	3.824	37	1.026
13	3.625	38	.959
14	3.441	39	.895
15	3.270	40	.832
16	3.109	41	.771
17	2.958	42	.711
18	2.816	43	.652
19	2.682	44	.595
20	2.554	45	.539
21	2.433	46	.484
22	2.317	47	.431
23	2.207	48	.379
24	2.101	49	.327
25	2.000	50	.277

parabola, the hyperbola, the cardoid, the logarithmic curve, the cissoid, the witch, the conchoid, the lemniscata, the sine curve, the cycloid, and the various spirals, to mention but a few. It remains to select that curve which will, so long as the relative difference between the longer and the shorter of alternate paths to a goal remains constant, and regardless of their absolute difference, yield the same excitatory difference.

These conditions are, as a matter of fact, satisfied by the logarithmic curve, quite as the tradition associated with Weber's law prepares us to expect. A series of values based on the logarithmic principle and adapted to the present situation are given in Table 2. These values have been plotted accurately, and appear in Fig. 5. Just as with the two previous gradients considered, the 5- and the 10-unit

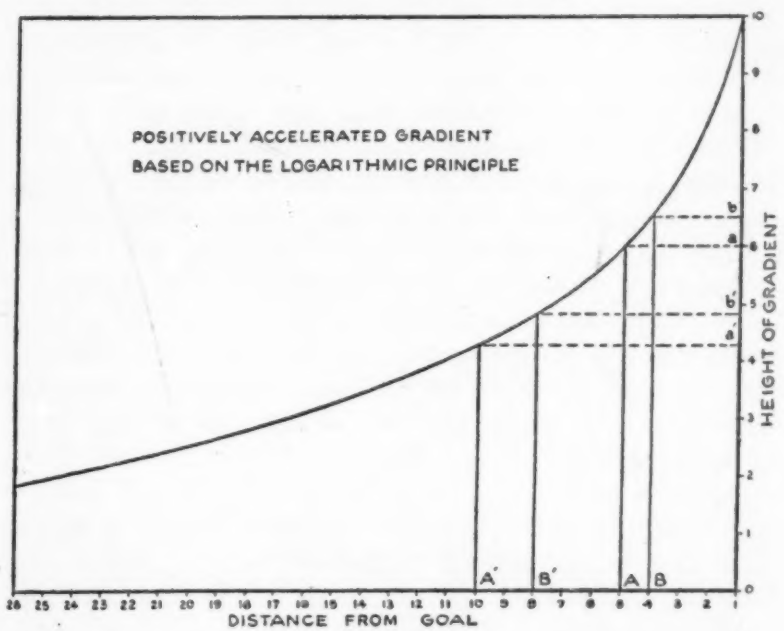

FIG. 5.

discrimination problem has been projected upon this logarithmic curve. Inspection of the diagram shows that the difference in excitatory tendency at the beginning of each pair of paths is sensibly the same. Reference to Table 2 yields

numerical values to the same end. These are summarized in Table 3. Yoshioka's experiment thus indicates that the goal

TABLE 3.

	Small maze	Large maze
Long path excitatory strength	6.554	4.831
Short path excitatory strength	6.00	4.277
Gradient difference	.554	.554

gradient of excitation in maze learning of the rat is one of positive acceleration and that it conforms very closely to the logarithmic law.

A corollary deducible immediately from Table 2 is that *the smaller the relative difference between two alternative mazes, the more difficult will be the discrimination.* Thus, in the case of the 10-unit maze just considered, a difference of one unit or one-tenth yields a gradient difference of .262 point, whereas a coarser one of two units, or one-fifth, yields the larger excitation difference of .554 point. The general *a priori* probability of such an issue as deduced from the above table is fully substantiated by Yoshioka's experimental results. He found that with a constant standard alley of 211 inches, a difference of 65 inches yielded a discrimination value of 18.85, one of 52 inches yielded a discrimination value of 17.65, 39 inches yielded 14.74, 26 inches yielded 11.89, and 13 inches yielded 10.80. Upon this basis we venture the fundamental generalization that *the larger the gradient difference between two alternative tendencies, the more perfect the preferential differentiation; whereas the smaller the gradient difference, the less perfect will be the differentiation.*

III

We may now proceed to the consideration of the problem of the elimination of blind alleys from the final path in maze learning. In general the operation of the goal gradient mechanism presents the elimination of blind alleys as a special case of the preference for the shorter path. At any point in the maze, as point *B* in Fig. 6, the animal has the choice of (1) proceeding directly to the food at *F*, or (2) going down the blind to *B'*, back to *B*, and then down to *F*. Clearly the direct path is the shorter. Other things equal, it follows at

once from the theorem developed in the preceding section that in the competition between the two paths, the one including the blind alley must be eliminated.

The conclusion thus arrived at on general principles may be demonstrated quantitatively. In the maze of Fig. 6, the blinds and the intervals between blinds are assumed all to be of equal traversing length. The distance between blinds will

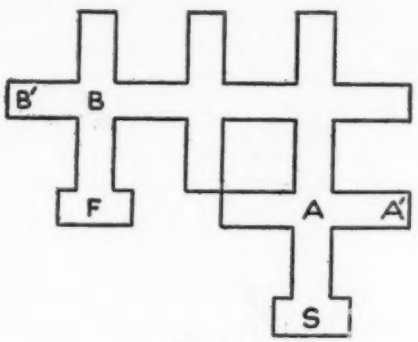

FIG. 6.

accordingly be taken as the unit of space traversed. Thus it is one unit from *B* directly to *F*. If, however, the animal goes from *B* to *B'*, then back to *B*, and finally to *F*, the distance traversed will be approximately three units. Referring to Table 2 we find that the goal gradient or excitatory tendency to acts leading directly to *F* has a value of 10.00 points, whereas the excitatory tendency leading to acts which are the beginning of a different behavior sequence and one three units distant from the goal is 7.270 points. In this conflict of excitatory tendencies, the one leading to the direct path is obviously prepotent with an excitatory margin of 2.730 points. Thus will the blind alley be eliminated.

In a similar manner it may be shown that in so far as this particular mechanism is operative a long blind alley will be eliminated more readily than a short one. Suppose for example, that blind *B'* were two units in length instead, of the one unit as shown in Fig. 6. To traverse this 2-unit blind on the way to the goal would add four units to the one unit of the true path, making five in all. The gradient

value for one unit is 10.000; that for five units is 6.000. There is accordingly available for the elimination of the 2-unit blind 4 gradient points, as against the 2.730 points for a 1-unit blind. The greater gradient difference should produce a more ready elimination of the long blind.

The above deductions, while not necessarily demanding that the animal shall have as many cases of going directly to the food as of reaching it by way of the blind at B', may possibly demand that the number of occasions of entering the blind shall not exceed the number of direct runs by an amount more than equivalent to the 2.730 points of natural goal gradient advantage of the direct path. In this connection it may be pointed out that there appears to be some mechanism within the rat's organism which makes for spontaneous variability of response, and which thus tends to insure the rather full exploration of all possibilities until definite conditioned excitatory tendencies get set up which are strong enough to over-ride the spontaneous tendency to variability. Fortunately the bulk of this spontaneous variability of reaction precedes the setting up of the excitatory tendencies, at which time it should produce a fairly equal distribution of practice. This should enable the goal gradient principle to play an important rôle in bringing about the elimination of the blind alleys.

There is good evidence that if by special experimental means the animal were induced to go into the wrong alley a quite disproportionate number of times, the blind would still be eliminated.[6] It is believed that at this point a second principle of considerable importance enters, which may be called provisionally the principle of habit families, or preferential hierarchies. This principle is a kind of second order phenomenon, being itself dependent upon the goal gradient and the resultant preference for the shorter path. A number of hitherto puzzling maze phenomena appear to be explained by it. This is the second of the two major aspects of maze learning mentioned in the introduction to the present paper. Space is here lacking for further elaboration of the principle.

[6] J. A. Gengerelli, Preliminary experiments on the causal factors in animal learning, *J. Comp. Psychol.*, 1928, 8, 1–23.

IV

We may now inquire the significance of the goal gradient hypothesis as regards the order of elimination of blinds in maze learning. The implication for the rat is very clear. In so far as this principle is operating it should produce an elimination of blinds first at the goal and then progressively backward towards the beginning of the maze. Consider, for example, the relative margin of excitatory strength furnished by the goal gradient for the elimination of blind alley *A'* at the beginning of the maze (Fig. 6) as compared with blind alley *B'* near the goal and already considered. Point *A* is 4 units distant from *F* by the direct path, whereas it is 6 units distant by way of *A'*. Referring to Table 2 we find that the excitatory tendency to movements in a sequence 4 units distant from the goal is 6.554 points, whereas that 6 units distant is 5.547 points, a gradient difference available for blind elimination of 1.007 points. We have already seen in the last section that the gradient advantage available for elimination of blind *B'* was 2.730, or over twice as much. Clearly other things equal, *B'* at the goal should be eliminated more readily than *A'* at the beginning of the maze. By means of Table 2, anyone interested may easily verify this principle with other values. The goal gradient hypothesis clearly operates in the direction of the backward elimination of blinds in maze learning.

By a simple extension of the above reasoning, there may also be deduced from the principle of the goal gradient the well-known fact that long mazes in general require more repetitions to learn than do short ones. Thus in Table 2 the difference in gradient made by entering a blind at the beginning of a maze is 2.730 points, at ten units from the goal it is .453, at twenty units from the goal it is .237 point, at fifty units from the goal it shrinks to .098 point. It is likely that for any given organism there is a gradient threshold below which the gradient difference corresponding to a blind alley may not fall if the blind is to be eliminated. This amounts to a deduction from the principle of the goal gradient that a maze may be too long to be learned at all.

A further extension of the above reasoning is that the steeper the goal gradient the more rapid the process of elimination, whereas the flatter the gradient the slower the elimination of errors. At the outset of the learning process the entire gradient is of course uniformly flat. The basic part of the learning process would seem to be the rise of the excitatory gradient. It should be possible to deduce the curve of the rise of this function with continued practice from the joint consideration of the nature of the mathematical characteristics of the goal gradient and the rate of elimination of blinds at the different stages of practice. Once the characteristics of the curve of acquisition of the goal gradient has been determined, numerous new relations may be deduced which will open the way for still further experimental testing of the hypothesis, with resulting additions to our knowledge of the learning process.

V

Just as in Section II we had occasion to inquire the implication of the three type gradients upon alternate maze-path discrimination, so now we may ask a similar question regarding the implication of the two types not yet considered as to the order of blind alley elimination. The answer is quite clear and unambiguous. A glance at the uniform gradient of Fig. 3 shows by simple inspection that for constant differences in the length of the maze path there will be equal amounts of excitation difference at all points of the maze. This means that blind alleys at one point will be eliminated, so far as the goal gradient is concerned, exactly as easily as at any other point. The curve of negative acceleration, however, presents a somewhat more interesting situation. An inspection of Fig. 4 shows at a glance that by this principle the nearer to the beginning of a maze, the greater the difference in goal gradient for a given distance traversed and, hence, the more readily will blind alleys be eliminated. In a word, *a negatively accelerated goal gradient should produce elimination of errors in a forward order.*

Two major combinations of the positively and negatively accelerated gradients are conceivable. One case would be

where the gradient begins as a positively accelerated curve but at some point in the sequence passes over into a negatively accelerated gradient. Such a combination gradient would produce the curious result of most rapid learning at some place in the midst of the maze and the slowest learning at the two ends. No such learning as this seems to have been reported.

The opposite combination would be a gradient beginning with a negative acceleration but reversing in the midst of the maze and finishing as positive acceleration. Such a gradient is shown in Fig. 7. This combination gives progressive elimin-

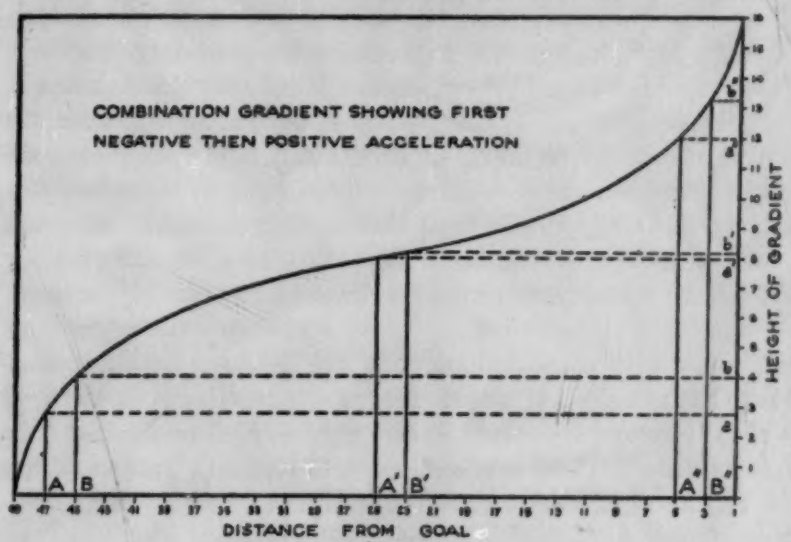

FIG. 7.

ation from both ends toward the interior with the slowest elimination at the point of inflection between the two types of acceleration. Thus the gradient difference for the elimination of blinds at AB and $A''B''$ is greater than at $A'B'$. This second acceleration combination is of special interest because it would generate what is believed by some to be the typical order of blind alley elimination of humans as contrasted with lower forms. If this should turn out to be true the interesting possibility would emerge that the basic type of gradient would be a combination one with the higher forms having the

inflection far up towards the goal, whereas lower forms would find their inflection progressively nearer the beginning, the rat in this respect possibly representing a limiting case. This combination gradient presents numerous intriguing implications as to learning phenomena, which can not be entered into at this time.

VI

There remains to be considered one of the most interesting of all the implications of the goal gradient hypothesis—the rate of locomotion through the different portions of the maze. Since in the ordinary maze the rate of locomotion is complicated by conflicts between two competing gradient tendencies which result in such irrelevancies as blind alley elimination in a characteristic order, such mazes are hardly suitable for testing the principle under consideration. The simplest case is where the animal is running from one end to the other of a long straight alley with food at the termination of the run. Under such conditions, since according to the goal gradient hypothesis the excitatory tendencies are progressively stronger as the goal is approached and since response is more vigorous and rapid with increased stimulation, it is reasonable to suppose that the animal would show a progressively more rapid rate of locomotion from the start to the finish of the run. In other words, if there is a well-marked stimulus gradient from the beginning of the maze to the goal, there should be a speed-of-locomotion gradient in some sense paralleling it throughout. This matter is of sufficient theoretical significance to be well worth an attempt at verification. A straight alley forty or fifty feet long with provision for the automatic recording of the moment at which the animal passes each of a half dozen points located at uniform distances through its length, should suffice as a basic apparatus.

A second test of the goal gradient hypothesis from the angle of the speed of locomotion is found in the relative rate at which animals move in the early portions of alternative-pathway mazes such as that shown in Fig. 1. Place easily visible doors at the entrance to paths *X* and *Y*. In the experiment let one door be closed and the other open. Determine

the length of time required by the animal to pass from *S* to the entrance of alleys *X* and *Y* respectively. Since, according to the goal gradient hypothesis, the strength of excitation to action is less on the longer path, we should expect a perceptibly slower pace from *S* to the entrace of *X* than from *S* to the entrance of *Y*. In a similar manner the time required for the animal to traverse the length of the first straight section of paths *X* and *Y* respectively could be determined experimentally. Here again, the goal gradient hypothesis demands that the speed of locomotion in the first section of path *X* should be slower than in the parallel section of path *Y*.

By way of contrast, a similar determination could be made for the final parallel sections of the two paths. Here, since each is the same distance from the goal, the gradient should be the same for both paths. The hypothesis accordingly demands that the speed on both paths should be approximately the same, except as it might be influenced by possible warming-up, fatigue, and similar effects.

Finally there may be considered the influence on the speed of locomotion produced by the conflict of the excitatory tendencies where a blind alley is in process of being eliminated in favor of the direct path. In this connection we may recall the values involved in the competition which resulted in the elimination of blind alley *A'* of the maze represented in Fig. 6. We saw that at point *A* there came to be an excitatory tendency of 5.547 points strength to movements in the direction of *A'*, whereas the excitatory tendency to going straight ahead on the direct path amounts to 6.554 points. Clearly if these two tendencies were equal and no other factor of importance were involved, they would neutralize each other and there would be no action, *i.e.*, the animal would pause at *A*, the point of choice.

In a more advanced stage of the evolution of the goal gradient (learning), where the gradient difference between the alternative tendencies has risen above the response threshold, it is reasonable to expect that the speed of locomotion at point *A* will be perceptibly reduced. If the two competing tendencies are positive quantities and fairly equal in size, the

difference between them available for motivation must be smaller than either. And since speed and amplitude of response is a function of the strength of the stimulus, there should be at first a tendency to slow down in the speed of locomotion as the animal passes the entrance of a blind alley previously eliminated.

VII

We may now survey the results of our inquiry. From the hypothesis that there exists an excitatory gradient extending with positive acceleration approximately according to the logarithmic law in an upward direction from the beginning of a maze to the reward box, there may be deduced the following principles of behavior:

1. That the animal will tend to choose the shorter of two alternative paths to a goal.

2. That the greater the difference between the length of the paths (the standard path remaining constant), the more readily will the shorter path be chosen.

3. That the readiness of choosing the shorter path will not be affected by the absolute difference between the alternates, provided the paths to be discriminated maintain a constant ratio to each other. (Weber's law.)

4. That animals will come to choose the direct path to a goal rather than enter blind alleys.

5. That long blinds will be more readily eliminated than short ones.

6. That the order of elimination of blind alleys will tend to be in the backward direction.

7. That long mazes will be learned with greater difficulty than short ones.

8. That animals in traversing a maze will move at a progressively more rapid pace as the goal is approached.

9. That of two alternative paths to a common goal the animal will traverse the early section of the shorter path at a faster rate than that of the parallel section of the longer one.

10. That the final parallel sections of two alternate paths each of different length leading to a common goal will be traversed at approximately equal speed.

11. That animals after having eliminated a blind will tend to pause at its entrance while pursuing the shorter path.

To this list there may be added from previously published studies the deductions:

12. That under certain circumstances segments of pure-stimulus-act sequences will drop out, producing what is known as 'short-circuiting.'

13. That fragments of goal reactions will tend to intrude into instrumental act sequences, producing the phenomenon of ideo-motor action and directive or guiding ideas.

The fact that it has been possible to deduce ten or so important behavior phenomena from a single hypothesis can scarcely be due to chance. It is hardly accidental that a single principle should be able to explain in the above deductive sense, and thus to integrate, such diverse phenomena as the choice of the shorter path, Weber's law, the elimination of blind alleys, the order of their elimination, the rate of locomotion through the different portions of a maze, the short-circuiting of symbolic-act sequences, and the origin of ideo-motor action. It encourages the belief that the science of mammalian behavior may be entering an era of systematic development more nearly comparable with that of the older, more exact, and more fully systematized sciences.

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STUDIES IN THE THEORY OF PERCEPTION
I. THE CLEARNESS–CONTEXT THEORY

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A theory of perception, whether held implicitly or explicitly, involves more than the mere acceptance or rejection of a particular hypothesis. We cannot separate perceptual and other organic activities and hence whatever principle of explanation we accept for the former must influence our position with regard to the latter. That this is true is proved by the fact that an examination of a writer's treatment of perception will reveal very often his whole psychological system. The problems of perception are therefore basic to every other problem in psychology. It is highly likely, moreover, that in the field of perception will be found at least the clues to the solution of many psychological problems; for, in spite of the assertions of some behaviorists to the contrary, it is in the field of perception that it is possible to control and vary conditions to the greatest possible extent. Furthermore, the problems of perception lie most surely within the proper domain of the psychologist, since our colleagues in neighboring fields usually dismiss them with a wave of the hand. We are therefore in a better position to evaluate assumptions, methods and theories in the field of perception than in any other field of interest to the psychologist, especially since the foundations of many of our other problems belong primarily to some other science. My concern over the theory of perception springs from a belief that it is the touchstone of any psychological system, point of view, or interest. It is my purpose, therefore, to examine a number of leading theories of perception in the hope that a number of psychological issues of more or less general import may be clarified and advanced. The first theory to be discussed is the clearness-context theory.[1]

[1] The usual name of the theory is the 'context' theory but owing to the prominent part played by attention or clearness in the statement and applications of the theory, the longer name has been adopted here.

The clearness-context theory of perception has been almost universally accepted both in this country and abroad by psychologists of widely different psychological faiths. While the terminology in which the theory is stated may vary from writer to writer, the underlying logic will be found to be the same in its varying presentations. By making due allowances for differences in terminology, the clearness-context theory may be found in the writings of behaviorists as well as introspectionists, in the writings of members of schools as well as in the writings of psychologists who vow allegiance to no particular school.[2] In this country the theory was presented most clearly and forcibly by the late Professor E. B. Titchener, who undoubtedly was influenced by the treatment of perceptual phenomena found in Wundt, G. E. Müller, and Hering.[3] But even if we grant that Titchener borrowed from these men, it is to him we must look for the most perfect expression of the clearness-context theory. It was he who stated its assumptions most clearly, held to them most tenaciously, and accepted their implications most unflinchingly. By discussing the theory for the most part as Titchener presented it we shall not be in any danger of omitting anything fundamental, since the numerous variants of the theory found in most of the textbooks of the last twenty years add nothing in the way of theory or application, fundamentally.[4]

[2] The extent to which this theory has dominated thinking and writing about perception can hardly be overestimated. It has even been adopted by philosophers who seem to think that its method of approach is the only one possible. The logic of the theory is so simple and, at first sight, so reasonable that is appeals to theorist and student alike.

[3] No further reference to these three writers will be given in this article, since a special consideration will be given to Müller's theory in a later article. We have chosen Titchener as the representative of the theory because it was he who influenced American psychology most directly, although most of Titchener's concepts can be traced to Wundt. Hering had no clearly formulated theory of perception, being in fact a physiologist. (See Note 30, p. 63, for reference to his theory of 'memory color' adopted by Titchener.)

[4] For Titchener's theory of perception we must go primarily to his books where he stated it explicitly and applied it to numerous phenomena. Since the last of his books was published some years before his death (except for the posthumous work edited by Weld) it might be doubted if his latest or final views have been properly taken account of in our discussion. To this there are two answers: (1) Except for verbal changes Titchener's view of perception as given here remained unaltered to the end:

We shall therefore examine the assumptions behind the clearness-context theory of perception and endeavor to appraise its applications to various problems of perception. We shall evaluate it on the basis of its agreement with observed fact, as well as on the basis of its internal consistency.

Perceptions were defined by Titchener as "selected groups of sensations in which images are incorporated as an integral part of the whole process." What distinguishes sensation from perception is that "no sensation means; a sensation simply goes on in various attributive ways. . . . All perceptions mean; they go on, also, in various attributive ways but they go on meaningly. Meaning, psychologically, is always context; one mental process means another if it is that other's context." [5] Psychological description and explanation consist, therefore, in a reduction of these complex formations, perceptions, into their psychological atoms, sensations and images. Meaning, which is constituted by the context of sensations and images aroused by the stimulus, may be carried, after a sufficient number of experiences with a particular stimulus, by 'brain habit,' that is, the accruing sensations and images not directly aroused by the stimulus may drop out while the full 'meaning' is still evoked.

Turning to the clearness aspect of the theory, we find that

his conception of the task of a scientific treatment of perception, methods of analysis and everything fundamental to the logic of his position were not sufficiently changed from the time of the *Textbook* and *A Beginner's Psychology* to his death to change our interpretation; (2) this view is supported by the writer's contact with Professor Titchener during the year 1924–25 when he had the pleasure of hearing him lecture and attending his seminary. The notes of Titchener's elementary lectures of 1924–25 show that his views had not changed sufficiently in the time elapsing between the publications just mentioned and this period to alter his presentation of the theory to students. Throughout this article the word 'sensation' has been used in preference to 'process' in spite of the fact that Titchener later (after Rahn's article) preferred the term *process* to *sensation*. If the word process is substituted for sensation both in Titchener's writings and in this article, no change in logic will be apparent. Nor can I see that his later swing to 'phenomenology' radically modifies his fundamental position, since his method first, last and always was to analyze by direct introspection. Furthermore, we are interested in the clearness-context theory in its many varieties and Titchener's clear-cut and original presentation serves as the model for most of the treatments appearing in current textbooks.

[5] E. B. Titchener, A textbook of psychology, 1909, pp. 364 ff., hereafter cited as textbook.

"even the simplest kind of perception . . . implies the grouping of sensations under the laws of attention . . . the process-attended-to becomes clearer and more distinct than the rest of consciousness. The processes-attended-from are rendered less clear and distinct." [6] While clearness, attensity, is merely one of the five attributes or dimensions of sensation or process, the others being quality, intensity, protensity, and extensity, it plays a far more important part in the context theory of perception than any of the others, since it can be used to bolster the theory wherever necessary. It is the one dimension of experience which has been most frequently invoked to explain why the other dimensions do not conform to what is expected on the basis of an assumed correspondence between stimulus and the core of sensations aroused by the stimulus. Hence the name given to the theory in this article.

Before attempting any further examination of the clearness-context theory we must see what methodological questions are involved in its assumptions. The fundamental proposition upon which this theory of perception rests may be found in Titchener's statement of what he conceives the problem of perception to consist in: "to analyze these given perceptions, to discover their elements, and to formulate the laws under which the elementary processes combine." [7] This certainly seems to be a simple, straightforward scientific way of attacking complex phenomena, comparable to the methods of physics and chemistry: complex perceptions are analyzed into simple elements and laws are to be formulated under which these simple elements combine again to give complexity. This analysis, it should be noted, is not the kind of analysis which proceeds by varying conditions and postulating simpler entities and relations to account for the complexes originally given, which we take to be the analytic method as practised in the other sciences, but it is rather an analysis which proceeds by direct inspection of the given, the method being to discover the simpler elements by a frontal attack upon

[6] *Idem*, Experimental psychology, 1901, vol. 1, pt. 1, p. 110.
[7] Textbook, 372.

the immediately presented data.[8] Moreover, the elements into which the complexes reduce have been fixed before the analysis begins, since by definition the elements of psychological processes are sensations and images. All perceptions reduce to combinations of these. And since the analysis must be limited to so-called psychological processes, the conditions, relations and whole physico-physiological matrix in which the observed data appear are either relegated to physics and physiology entirely, or play a very minor part in the description and explanation of the original perception.[9]

The assumption that the elements of experience [10] (sensations, processes, attributes, dimensions) have no meaning enables the proponents of the clearness-context theory to explain by one fell swoop a large variety of perceptions. For

[8] For many psychologists, following Titchener, there is and can be only one type of analysis of a perceptual phenomenon, namely, that which consists in a reduction of something complex (the originally given perception) into something more fundamental psychologically (sensory or other 'units'). Apparently these scientists never have realized that scientific analyses do not proceed in this *direct* fashion, that it is only when an analysis has been absolutely established that a direct disintegration of a complex phenomenon is possible. The method of analysis in science consists rather in a variation of the conditions in which a phenomenon appears so that after sufficient observations of the behavior of the phenomenon in question an analysis, in the first instance always *conceptual*, is made. Further experimentation establishes whether the analysis is correct or not. This same method can be applied to perception, in fact was done so by Wertheimer with the phi-phenomenon. But, blinded to any kind of analysis that didn't yield 'sensory processes' certain psychologists refused to regard Wertheimer's results as in any sense analytical, and proceeded to find the last, ultimate, sensations composing movement. A good test of any kind of analysis is this: does it enable me to control, predict and relate to other phenomena a given datum?

[9] The strict Titchenerian, of course, denies that conditions, physiological processes (either actual or assumed) have anything to do with psychological description, even though Titchener asserted that the explanation of a psychological event is to be sought in the nervous system. Who wishes may subscribe to a pure description of mental process, divorced from stimulus and organic conditions but let him remember that he has arbitrarily selected one part of a total process for description, since the percept cannot be dissociated from what is perceived. A scientific account must include everything *relevant* to a given event. Can an introspectionist deny that the stimulus and organic conditions are irrelevant to the mental event? Then why should it not form an integral part of the psychologist's account? This means that the science of psychology has grown from a mere cataloguing of sensations to a study of functional events co-terminal with the events studied by her sister-sciences.

[10] For the best account of the part played by 'meaning' see A beginner's psychology, 1923, pp. 26–30. (This book will hereafter be referred to as Beginner's.)

if the stimulus-complex arouses a number of elementary processes which normally evoke a given perception, it follows that any deviation in the perceptual processes when the stimulus-complex remains the same, must be due to a change in the 'meaning' which the psychological processes carry and not in the components of the perception themselves. A one-to-one correlation between the elements of perception and certain aspects of the stimulus-complex is thus assured and whenever the correlation normally expected fails to appear, it is asserted that nothing fundamental has changed in the sensory processes. All differences that occur when different *O*s perceive different things when the stimulus-complex is the same for all, and different perceptions of the same stimulus-complex by the same *O* are attributed to differences in 'meaning' or what has accrued to the sensory processes aroused by the stimulus. The constancy of perception is taken care of by the assumption of a constant core of sensations aroused by the stimulus, and the lack of constancy in perception is explained by the variety of meanings which may attach to a given core of sensations. Meaning thus plays a dominant rôle in the clearness-context theory—indeed, context constitutes meaning, while the elements of the context represent psychologically real and rather unchangeable elements.[11]

The concept of clearness more than any of the other so-called dimensions of sensations, helps to bolster the context theory at a number of weak places.[12] Various parts of a perception have different degrees of clearness at different times. If a group of sensations does not give rise to the kind of perception expected, recourse is had to clearness: either the contents themselves are unclear (attributive unclearness) or

[11] Titchener undoubtedly had before him the ideal of the fixed chemical elements, overlooking the real method by which the chemist discovered his elements and the difficulties of dealing with complexes in chemistry either conceptually or by actual synthesis. See Textbook, pp. 49–50.

[12] Wundt needed attention not only as a descriptive term, clearness, to complete his theory of perception, but also as an activity or 'inner will-process.' Titchener uses it only descriptively, as clearness-grade attaching to contents. For Wundt, see Grundzüge d. Physiol. Psychol., 6th ed., vol. 3, 306 ff.; Titchener's final adoption of the descriptive view of attention is probably signalized in a note in *Amer. J. Psychol.*, 1924, 35, 156.

the meaning of the sensations is unclear (cognitive unclearness). Thus if an *O* steadily fixates one of the fainter rings of the Masson disc, it will alternately disappear and appear, showing, according to the proponents of the theory we are discussing, changes in clearness or 'fluctuations of attention.' Here the changes concern so-called attributive clearness, whereas if I fail to make out what a page of Russian print is about, it is because the meaning is unclear or the words are cognitively unclear.[13]

To follow the implications and show clearly the assumptions of the clearness-context theory it is necessary that we examine actual cases to which it has been applied. A survey of the literature shows that the theory has been used to describe and explain at least the following classes of facts: (1) deviations in perception from what is normally expected when the stimulus-complex does not change; (2) fluctuations in the appearance of a stimulus-complex when the stimulus-conditions are constant; (3) various types of 'illusions' in which the perceptual complex and the stimulus-complex do not exhibit sufficiently close correspondence to be classed as normal; (4) differences in individual reactions to complexes belonging to the ambiguous figure group which, in many ways, may belong to several of the other classes mentioned here; (5) cases involving the preservation of certain properties of a complexlike form, size or quality under conditions that would lead us to expect differences in these properties; (6) the perception of more than is given in the stimulus-complex; (7) the growth, and decay of perceptual processes in time; (8) the place of habit and 'past experience' in the perceptual processes.[14]

We shall first discuss those classes of facts to which the clearness-context theory has been applied that involve clearness primarily, then those which involve both clearness and context, and finally, the facts which have been explained

[13] For this distinction see K. M. Dallenbach, Attributive *vs.* cognitive clearness, *J. Exper. Psychol.*, 1920, 3, 183–230.

[14] The classes of facts contained in this list are neither exhaustive nor mutually exclusive. They represent what seem to be the chief classes of data to which the clearness-context theory has been applied. Undoubtedly there are others.

chiefly by the concept of 'meaning,' 'interpretation' or context. Since various aspects and assumptions of the theory are brought out in different applications to concrete problems, those applications will be discussed which seem to the writer to bring out most clearly the various issues involved.

CASE I.—Under this application of the clearness-context theory the attempt has been made to explain why a figure which may be seen as one object or another appears as the one or the other when the stimulus does not change. This case has been subsumed by Titchener under his first law of attention: "The process attended-to becomes more clear and distinct." Success in finding a concealed object in a puzzle picture comes from the fact that the object sought has attained a greater degree of clearness. The brain-babies figure given by Titchener in his Textbook [15] furnishes an ideal illustration: at first sight the drawing seems to be a picture of a human brain but upon closer inspection it turns out to be a mass of intertwining bodies of babies. According to Titchener's account of this phenomenon when the picture is seen as a brain, the babies are unclear and when the babies are seen "the diagram becomes fairly alive with them, and the brain-perception is reduced to a very bare and vague schema." The change from the one to the other is, therefore, a change in clearness grade attaching to each: "Suddenly you find what you are looking for . . . the picture of the brain drops clean away from the upper level: the concealed outlines stand out with all imaginable clearness, and the form of the brain is no clearer than the feel of the book in you hand." [16]

This account of the brain-babies picture implies that when the brain is seen the babies are also seen, only at a lower level of clearness and that when the babies are seen, the brain is present at a lower level of clearness. If this is true, why do not *O*s report, when they are asked what they first see in the picture, "A clear picture of a brain and unclear babies"? Why, if the babies are present at all, is (as Titchener points out) a *search* necessary to find the babies when the picture is

[15] Textbook, p. 278.
[16] *Ibid.*, 277 ff.

seen as a brain? The assumption obviously is made that the only change when the babies are perceived is a change in clearness, the other attributes of the sensations remaining more or less constant. This is neither more nor less than the constancy hypothesis, for it is assumed that while the brain is present the picture of the babies is present also, a mere change in clearness sufficing to bring the latter out. But it requires little argument to show that the change from brain to babies is more than a change in clearness, because at its best when the brain is seen there is no hint of babies at any level of clearness whatsoever. To assume that the perception of brain included the perception of babies when the latter were not reported, is to substitute theory for fact and to withdraw the whole problem from the test of observation.

CASE 2.—Another great class of facts, known as the illusions of reversible perspective, of which Mach's book, Necker's cube, Schröder's staircase figure, and Hering's crosses are examples,[17] has been described as if changes in clearness were responsible for the phenomena reported. Although some of the effects have been attributed to context as well, we shall discuss only the use of clearness in these cases, since other examples illustrate better the applications of the concept of context. Taking the Necker cube as typical of these phenomena, it is found that either the smaller square in the center of the figure may be seen toward the *O*, or the larger, outside square. By gazing fixedly at the figure fluctuations occur in which now the one part is toward the *O*, now the other. According to the clearness account, when the smaller square is toward us it is clear and the larger is unclear, and when the larger square is toward us it is clear and the smaller is unclear. Changes from convex to concave and *vice versa* according to this description merely involve changes in clearness at bottom. We may judge of the adequacy of this description by two criteria: (1) the reports of *O*s to whom the figure is submitted; (2) the implications of the clearness treatment. Having asked a number of *O*s to state fully what

[17] The greatest variety of figures falling in this class will be found in Titchener's Experimental psychology, 1, i, 153 ff.

occurs when the two squares change positions, I have found that all agree that now the one square is nearer, now farther away, but that *both are at all times clear*. In other words, a concavity can be and is just as clear as a convexity. The implication contained in the clearness theory that when a part of the figure is concave or away from the *O* it is unclear, is untrue. The weakness of the statement in terms of clearness is at once appreciated if it is realized that it implies that a clear convex appearance is the same as an unclear concave appearance, since, by hypothesis, when the square is convex it is clear and when it is concave it is unclear. But why cannot an appearance of concavity be as clear as an appearance of convexity? Titchener himself has given a description of this particular figure in terms of convexity and concavity of parts,[18] but in his general description of these cases the concept of clearness is invoked as a more fundamental and general term. The appeal to clearness springs from the dictum that all psychological description must be in terms of attributive or sensory processes. Terms like concave and convex which have not been admitted into the company of scientific concepts find no place in the basic descriptions and explanations of the good Titchenerian.

If it is believed that the term *clearness* possesses special claim for admission into the select company of psychologically admissible terms, the following two cases to which it has been applied will amply demonstrate that as a concept it has been by no means clear and leaves much to be desired in the way of describing accurately and univocally the various properties of perception it is supposed to include or to displace.

CASE 3.—The classic case that has been described as a change in clearness is the Masson disc phenomenon, and yet no better example exists of the inadequacy of the clearness concept than this use of it. If one of the fainter rings of the Masson disc is fixated, one sees a faint gray ring on a white ground, which more or less gradually disappears into the white background, reappearing later only to disappear again, and so on. Titchener,[19] with a host of others, has asserted that

[18] Experimental psychology, I, ii, 310 ff.

[19] Textbook, p. 292.

the change from the gray ring to white ground involves a change in clearness, since the phenomenon was believed to spring from a 'fluctuation of attention.' The clearness theory obviously implies that when the gray ring is seen, the white ground is unclear, and when the gray ring disappears, it is unclear while the ground has become clear, the changes being merely a reversal in the clearness grades of ring and ground. Again we are led to the absurd conclusion, absurd from the standpoint of observation, that an unclear gray ring is a clear white ground, and a clear gray ring is an unclear white ground. Stated in this form no one would accept the clearness description; and yet the account given in most of the textbooks (following Titchener) implies this absurd statement, which has only to be made explicit to be absolutely rejected.

The clearness account suffers from ambiguity in its pretended description of the fluctuating gray ring, for to say that when the gray ring has disappeared it is unclear, does not tell us what is actually present in perception. An unclearness may comprehend an infinity of objects, since all unclear objects are more or less similar. When the gray ring disappears a homogeneous white ground appears in full clarity, when the phenomenon is seen at its best, but it is obvious that the description in terms of clearness contains no hint of the white ground actually observed.

The final test of any theory lies in observation. To discover how well the theory agrees with observed fact, I have asked a number of *O*s to report carefully what is actually given in perception during the so-called fluctuations of attention with the Masson disc as stimulus. The reports resolve into the following three types of description: (1) when the gray ring disappears, no trace of it is present, the field being a homogeneous white; in this optimal case there is no question of an unclear gray ring; (2) the gray ring does not completely disappear, but becomes fuzzy at its edges, lighter in tint, or washed out, some *O*s asserting it is unclear; (3) other *O*s who describe the ring as in (2), maintain that the ring is still clear, only changed in appearance. Less than one-third of all *O*s positively report the ring as unclear, and probably this num-

ber would be reduced if students were not introduced to the clearness account of the phenomenon by their texts.

We must therefore reject the clearness account of the fluctuating Masson rings on two counts: (1) it does not square with actually observed facts; (2) it blankets a number of different observations under a single descriptive term which is ambiguous, and implies this single characterization is descriptively adequate to encompass a variety of phenomena. While it is true that science seeks to reduce the many to the one, it is better to err on the side of richness and variety in description at the beginning lest our science become thin and poor at the end.

CASE 4.—Among the so-called reversible figures, the types reintroduced and emphasized by Rubin under the title of figure-ground phenomena has been subsumed to the clearness-unclearness phenomena by a number of recent writers, in spite of the evidence presented by Rubin to show that the older descriptive terms are inadequate as a designation of its most important features. One hardly needs to describe the Rubin figure, since it is so well known: one sees either a white vase or goblet against a black ground, or a pair of faces in silhouette against a white ground.[20] When one is seen, it is called figure and the rest ground, although, as Rubin pointed out, it is possible to hold faces and goblet in view simultaneously. Wever in a recent study set himself the task of showing that the clearness-unclearness concepts apply to this phenomenon and believed that in the interests of 'descriptive adequacy' it is necessary to characterize the figure as clear and the ground as unclear. We shall examine his position in the matter as a typical case of the attempt to apply the clearness-context theory to phenomena of this type.

Wever maintains [21] that "the reversal of ambiguous stimulus-fields is found to be accompanied by a reversal of the order of clearness-levels" on the basis of reports of *O*s who were

[20] The original figure was published in Rubin's book, Visuell wahrgenommene Figuren, 1921, and reproduced by E. G. Wever in an article entitled, Figure and ground in the visual perception of form, *Amer. J. Psychol.*, 1927, 37, 194–226.

[21] E. G. Wever, Attention and clearness in the perception of figure and ground, *Amer. J. Psychol.*, 1928, 40, 51–74.

asked to describe the changes occurring when a field was figure and when ground. Since the *O*s were instructed to report upon changes in grade of clearness, it is not surprising that a high correlation was found between clearness and figure, unclearness and ground. But had the *O*s been instructed to report on any one or more of a dozen other antithetical properties which have been found to characterize figure and ground, high correlations would have been found among them as well. Why then did Wever set himself the task of correlating clearness-unclearness and figure and ground? In the interests of 'descriptive adequacy' we are told, because Rubin had denied that the essential difference between figure and ground is one of grade of clearness attaching to each. But what constitutes 'descriptive adequacy'? Why is clearness any more descriptively adequate than 'structuration,' 'thingliness,' or a dozen other terms which have been used to designate the difference between figure and ground? Wever does not tell us.

It might be thought that the choice of clearness was made because it is an unambiguous term, meaning the same thing to all *O*s and irreducible to anything more fundamental. Wever's protocols do not show that clearness has any precise meaning or that it even means the same thing to the different *O*s who were forced to use the term. Thus among the meanings given to the term clearness in Wever's study we find that it may mean 'centrality in the field of experience,' 'memory value,' 'definite value with respect to reportable attributes' or 'the presence of something that can be reported upon.' One *O* reported that "I might say that it is 'figurehood,' but I am wary of that as 'blanketing'".[22] 'Figurehood' is blanketing while the term 'clearness' is not. Why the one term, used by Rubin, should be regarded as any more 'blanketing' than the other word, used by Wundt and Titchener, is hard for anyone but a Titchenerian to understand. It appears as if this writer prefers terms characterizing the so-called attributive aspects of perception, and fears to recognize any which have not yet been admitted into that company. In the

[22] *Ibid.*, pp. 66–67.

endeavor to 'be on the safe side [Titchenerian?] of adequacy' he clings to the concept of clearness as 'the clearness of experiential content.' [23] The assumption seems to be that one is only safe when describing experience from the point of view of the rock-bottom elements that compose it, be they sensations, processes, dimensions or attributive categories.

It is not my wish to press a position beyond the limits fixed for it by its defenders. It should therefore be noted that Wever refuses to accept the Titchenerian dichotomy of process and meaning, carried over into the concept of clearness, to distinguish so-called sensory or attributive from cognitive or meaning clearnesses.[24] Wever apparently stands for only one kind of clearness, differing only in grade, thereby refusing to make the distinction between meaningful and non-meaningful processes. But he still seems to cling to the foundation-stone of the whole Titchenerian scheme when he argues for a concept that shall be applicable to 'experiential content,' 'to be on the safe side of adequacy.' It is difficult for a non-Titchenerian to understand why the fact that the figure is clearer than the ground (a fact which Rubin has denied on the basis of his *O*s' reports) should be more characteristic or more descriptively adequate than the fact that the figure 'has more structure in it,' as one of Wever's own *O*s reported, that it is more thingly, that it possesses a higher limen for color induction than the ground, that the boundary seems to belong to it more than to the ground, or the fact that it is remembered better.

In support of the clearness account of these differences in perceptual fields, Wever has asked for and given a reconsideration to the concept of clearness in the belief that it may be 'a very real and significant affair,' yet nowhere does he show how or why a statement in terms of clearness-difference is any more significant or real than any of the others reported by Rubin's and his own *O*s. It is hardly necessary to repeat the criticisms levelled against the clearness-unclearness account of figure-ground phenomena. If it can be shown that the clearness quality implies more, is a more fruitful basis for

[23] *Ibid.*, pp. 55; 74.
[24] *Ibid.*

further work, or possesses greater explanatory or descriptive significance than any others, a real service will be rendered to psychology.[25]

CASE 5.—We come now to applications of the clearness-context theory which involve the use of context rather than clearness, although the latter term is often invoked to help explain these perceptions. In this class of perceptions the chief fact to be accounted for is why so-called sensations do not give rise to the perception expected on the basis of a knowledge of the stimulus-complex. Sometimes the lack of constancy between perception and stimulus is explained by an appeal to clearness, on the assumption that a given stimulus-complex always arouses the same complex of sensations. Thus one recent writer says 'the sensory impressions are always what they are and what they ought to be,' differences from the expected, often called illusions, springing from 'indistinctness (unclearness) of the impression or its faulty interpretation.'[26] If this statement of the case is true, then sensory processes bear a one-to-one correlation with the stimulus-complex, and so-called sensory processes should be found to be univocally determined, once the stimulus-conditions are determined. To bolster the constancy hypothesis, when the expected constancy fails to be realized, it is asserted that the complex of sensations has been faultily interpreted, carries a different meaning, or is unclear. Let us take a typical example of this application of the theory.

[25] It is interesting to note that of the many important differences between figure and ground, the only one that most American psychologists have chosen is that of clearness grade attaching to each! This is but one bit of evidence of the hold that descriptions and explanations in terms of 'clearness' have on the writers of textbooks. Such interesting and important functional facts as the following receive no mention: a figured field was shown by Rubin to be remembered better, to have a higher limen for color induction, to condition recognition, *etc.*, *etc.* So strong has the tradition been against admitting functional facts or facts concerning the everyday organism into the science that whatever does not smack of 'existence' (as opposed to use or value) finds no place in the science. Probably no science has succeeded in being straight-jacketed and its findings emasculated so well as psychology.

[26] F. H. Lund, Psychology, 1927, pp. 333–334. I do not mean to single out this writer,—in fact his statement of position has the merit of being clear and univocal and offers chance of discussion. Similar beliefs are implied in practically every textbook account of perception published in the last 25 years. The more recent books, with one or two rare exceptions, are just as incorrigible as the earlier.

The object held in my hand (a pen) consists of a complex of hues, tints, chromas, tactile and imaginal processes psychologically. To me it is 'my pen' but to others it is 'his pen.' Why the difference in reaction, or meaning? The usual explanation given asserts that images from my past experience which accrue to the sensations furnished by the stimulus are different from the images and secondary sensations accruing to your complex of sensations aroused by the stimulus. Hence the difference in meaning to me lies in the different context aroused by the stimulus. The theory as applied here seems reasonable enough, especially since it appears easier to secure agreement upon what is given 'sensationally' to the various *O*s than meaningfully. And what seems more natural than to appeal to differences in past experiences to explain the difference in present meaning of a stimulus common to a number of *O*s?

While admitting there is some difference in meaning in the case of the pen there are probably no greater differences in meaning than in sensation when we come to examine the matter a little more closely. For not every *O* possesses the same color vision, tactile and imaginal processes that every other does, hence there must be differences in sensation even with a common stimulus. On the other hand, there is large measure of agreement on 'meaning,' for all agree that the object means 'pen'. There is no better evidence for the constancy of stimulus-sensation than for the constancy of stimulus-meaning once we have decided to question the constancy hypothesis for sensory processes. Individual differences have not been *proved* to be any greater for *imaginal* than for sensory processes. The fact that different images may 'mean' the same object is certainly as much of a problem for the proponent of the context theory as for anyone else. The attempt to explain this fact by reference to 'past experience' will be shown in the next section to have failed. By allowing meanings and processes to vary independently it has been possible to relegate a large number of problems to the wastebasket of meaning without further ado. If we allow individual differences in perception and reaction to be assigned to differences in

'interpretation', 'meaning' and the like, we lose our chance to bring within the circle of knowledge what is at present untouched and unknown.

Sometimes it is assumed that differences in context may change the sensory processes themselves, although this is contra hypothesis. Thus the difference in length of the central lines of the Müller-Lyer figure have been ascribed to the influence of kinæsthetic and other secondary sensations aroused by the visual stimulus. Here an inconsistency can be detected in a theory which originally assumed that the sensory processes aroused by the stimulus 'are what they are and what they ought to be,' when it is admitted that the altered length involves a change in a sensory dimension, namely that of extent. Furthermore, it is utterly incomprehensible how non-visual processes become incorporated into the visual perception to effect a change in the *seen length* of the lines. The difficulty lies, of course, in assuming that merely added processes can account for the lack of constancy between stimulus-complex and perceptual-complex without affecting materially the basic sensory processes. When, as in this case, the alteration concerns processes that are undeniably 'sensory', recourse to 'interpretation', 'meaning' and context fail to satisfy. Nothing short of complete renunciation of the constancy hypothesis will do here because the sensory processes themselves fail to exhibit the constancy that characterizes the simpler cases.

Case 6.—The part played by past experience in furnishing a context to the sensations supposed to be aroused by the stimulus in the clearness-context theory appears in the ambiguous figure of a duck-rabbit head which is typical of a large class of perceptions. In order to account for the fact that in looking at this figure one person sees at first glance a duck's head, another a rabbit's head, it is asserted that if past experience favors the one it will be seen, if the other, that will be seen. Thorndike, in referring to this example, has stated that "The same outside thing may arouse different percepts . . . in different people according to the amount and nature of their previous experience of it." [27]

[27] E. L. Thorndike, The elements of psychology, 1917, p. 36. The duck-rabbit figure will be found on p. 41.

The suggested explanation trenches upon a truth which is so obvious and trite and general that it is of little use. Of course past experience has something to do with present reactions, but the concept of 'past experience' in itself does not tell us precisely what we want to know: how and when will two individuals react differently to the same stimulus-complex? It is a truism that the past experiences of no two individuals are alike, and to explain differences in present reactions by an appeal to past experiences which are different is merely to appeal to individual differences in order to explain individual differences. No better example of arguing in a circle could be given.

On the other hand, what is meant by past experience? Some writers obviously regard the repetitive factor in past experience as the main source of its power of reproduction, others regard vividness, still others recency, and a few still hold that the consequences or affective states aroused by an experience favor its reappearance. As a general concept stated *simpliciter*, past experience involves the fallacy of *Secundum Quid*, which consists in using a principle without regard to the circumstances which modify its applicability in given concrete instances. When precise meanings are attached to the concept like frequency, recency and vividness, it is found that no one of them suffices to explain all cases. Another use of the term consists in discovering by a process of questioning or analysis something in the individual's biography which seems to account for the present reaction; but even here the pretended explanation sounds very much like *post hoc, ergo propter hoc*. Since past experience contains so many possible relations to any present experience its very wealth of detail proves an obstacle to its use. Scientific concepts are valuable not only as they are generally applicable but also to the extent to which they provide differentia by which specific instances are delimited and defined.

The real question which few writers employing the concept of past experience have faced is: "How does past experience change an organism so that a given reaction takes place under a given set of conditions in the present?" In a

given instance we must know (at least in theory) what *present* mechanisms are operative to produce present reactions. The concept of past experience is in large measure responsible for turning attention away from many important problems in behavior by its easy blanketing and superficial explanations of diverse problems.

It is hardly necessary, to return to the illustration with which we began this section, to point out how little is really added when we say that the past experience of an individual will account for his perception of duck or rabbit when presented with the ambiguous stimulus containing both. If we try to predict a given individual's reactions armed with this concept as a guide, unless we know his complete life history, an impossible feat, then its emptiness is realized. Its use after the reaction is known smacks too much of the 'I told you so' brand of omniscience found in everyday life when would-be wise-acres proclaim their prescience. A satisfactory theory must give more definite information as to what is probably present at any given moment to account for an individual's reactions. If it is impossible for all the facts to be encompassed in one theory, then a number of different theories may be needed ultimately.[28] Past experience tells us too much as a general term, too little when made to mean something specific for concrete cases. Its emptiness unfits it for use except in drawing-room psychlogy.

Case 7.—The fact that colors, shapes and size tend to be maintained in the face of changing conditions has impressed most writers on perception and has given rise to a number of theories to account for the same. It presents special difficulties to the clearness-context theory because here the usual state of affairs is reversed: instead of the stimulus-conditions remaining constant and the perception changing, the stimulus-conditions have been found to change without a corresponding change in perception. We have here to account for such phenomena as color constancy, and the tendency of objects

[28] It is my belief that we have tried to make a single theory or point of view do too much in psychology. Probably several theories may ultimately be necessary to explain all the facts.

to preserve their 'normal' size and shape. Examples of each are not hard to find: paper that looks white under sunlight still looks white under illumination that should make it look yellow: a coin may preserve its perfectly round appearance in perception when the image on the retina is elliptical; objects do not grow smaller as they recede from us at quite the rate demanded by the law of visual angle.

Lest it seem as if certain aspects of the theory we are discussing have been over-emphasized, let us take as this example Titchener's use of the theory to explain why portions of the landscape falling on the peripheral retina resemble the parts falling on the fovea when it is known that the sensitivity of the retina is markedly different in the fovea from what it is peripherally. He says: "We are accustomed to turn our eyes directly toward what we see, and therefore to view it with all three daylight films . . . so that we totally disregard the altered look of things seen 'out of the corner of the eye.' Even if we do not, we are likely to *remember* how the things appear in direct vision; we paint them over, so to speak, with *memory colors*, colors that represent their natural or average appearance at the center of the visual field." Memory color leads us 'to overlook in great part the actual variation of lights and colors in the landscape.' [29]

Two assumptions have been made in the statement of this application of the theory which should be made explicit: (1) that the change in stimulus-conditions is paralleled in sensation, and (2) that the actual differences within the perceptual field go unnoticed. In the first we have a clear case of the constancy hypothesis and in the second we find the doctrine of unnoticed sensations used to support the constancy hypothesis. But, says Titchener, "even if we do not . . . disregard the altered look of things" (in the periphery of vision), 'memory color' paints over the heterogeneous field to make it appear homogeneous and as we have experienced it before.[30] Thus the perception of homogeneity when the stimulus- or sense-organ-complex would lead us to expect

[29] E. B. Titchener, Beginner's, p. 62.

[30] Titchener borrowed this concept from E. Hering in whose Grundzüge d. Lehre vom Lichtsinn, pp. 6ff., it will be found.

heterogeneity, and the 'normal' appearance of a field when we expect a deviation from the normal, are explained in last analysis by bringing in another element to the elements assumed to constitute the perception: a memory color, in the form of an image, added to the sensations aroused by the stimulus-complex, paints out the differences really present in sensation. When elements do not function as they should, according to the original assumptions, another element provides the way out of the difficulties facing the theory.

A test of this theory is easily possible. A sheet of bond paper which ordinarily looks white is shown to a number of subjects under lamp light in which red and yellow predominate so that the paper actually reflects more of these colors than any others (Hering's experiment). We should therefore expect the paper to be reported as yellow under these conditions. Hering was probably the first to find that the normal color may be preserved, and he invoked the concept of memory color to explain this fact. Reports from a number of *O*s with whom I have carried out the experiment show that there are actually three possibilities: (1) to some *O*s the paper still looks white; (2) other *O*s report it as yellow; (3) the rest give reports of uncertainty saying either that the paper appears yellow, then white, or that it has a surface sheen of yellow and a deeper, underlying white color. By means of the memory-image concept can we predict when the paper will appear white, yellow, or of uncertain color? When will the memory image be added to preserve the normal appearance of the paper, when will it fail to function, and when will it 'overlay' but not 'paint' the underlying color? How much are we helped by assuming that the 'sensory' processes are of yellow under conditions that would lead us to expect a perception of yellow when actual observations disclose a number of possibilities? Have we any more right to call the processes which we normally expect a stimulus to arouse 'sensory' than other processes which observation tells us are present? The moment we attempt to answer these questions we discover the purely *ad hoc* nature of the memory color explanation. As a theory it fails to meet one of the best tests we have of a theory's

truth, namely, power of prediction. A theory which can be made to explain events after their occurrence is worth little, since it is then possible to juggle terms arbitrarily to fit a given case. Our theory must enable us to handle every case, proceeding from known conditions to definite consequents.

CASE 8.—Under this class of perceptions we include all those cases in which more is experienced than can be accounted for by reference to the stimulus-conditions alone. These cases involve a 'filling in' or completion in perception of data that are incomplete on the stimulus-side. Illustrations of these completions are to be found in incomplete letters which give rise to complete words, partially-drawn outlines of the human form which are seen as complete, and in other types of figure wherein more is seen than greets the eye.[31]

Titchener has classified these completion phenomena under cases of 'tied imagery' because "they are unequivocally bound up with the sensory portion of the perception." In other words, when the sensory portion of the perception is incomplete an image from past experience becomes 'tied' to the sensory processes and completes them. Here the old assumption of the one-to-one correlation between stimulus and sensory processes crops out again, because if the stimulus-complex presents lacunae the sensory processes must *ipso facto* be incomplete. But observation fails to substantiate this assumption, as Titchener himself tacitly recognized when he said that the 'tied image' is "unequivocally bound up with the sensory portion of the perception." "Even a missing orchestral part . . . may be clearly recognized," we are told.[32] It would appear, therefore, as if the whole perception, in such cases, were on a 'sensory' level. Logical considerations alone seem to demand an image having all the characteristics of sensory processes and 'unequivocally bound up' with them, since observation fails to discriminate between 'sensation' and 'image'.

We soon encounter other difficulties. Not all *O*s have tied images, in fact only 'a large minority have tied images'

[31] Examples will be found in almost any textbook.

[32] Beginner's, pp. 62–63.

according to Titchener. But it is not denied that all *O*s experience completed perceptions at some time or other, so we must conclude that Titchener himself realized that his explanation applied only to a few of the many individuals who have completed perceptions.[33]

It is not necessary to point out assumptions made in this application of the theory which we have found in others. Suffice it to say that I find it impossible even to imagine how an image can become 'tied' to sensory experience. This would indeed be a matter for some pretty physiological speculation. By Titchener's own admission the completed portion of the perception forms so complete and intimate a union with the rest that anything short of a unitary underlying process that is responsible for the total perception seems unsatisfactory. If the completed portion of the perception appears with the same *Eindringlichkeit* that the rest does, then we have no right to ascribe one part to imagination and the other to sensory processes in order to save the constancy hypothesis.

Case 9.—Our discussion of the clearness-context theory thus far has indicated that it envisages the formation of complexes through the addition of simples and that it regards certain portions of the perception, the so-called sensations or sensory processes, as being in one-to-one correlation with the stimulus or certain aspects of the stimulus-complex. Differences in perceptions have been explained through the addition of imaginal elements which modify the sensory processes or attach a different meaning to the whole. It might be objected that our statement of these assumptions does not represent the clearness-context theory fairly, that possibly certain aspects of it have been over- or under-stated. Titchener's six proofs for the separability of processes or sensations from meaning to account for the growth, decay and variability of meanings, show that we have not given the clearness-context theory a more atomistic interpretation than its inventor himself gave to it.

Nothing can show better the purely additive way in which

[33] *Ibid.*

Titchener regarded perception than his six proofs for the separability of process and meaning which are as follows: (1) meaning may be stripped from the mental process to which it normally belongs; (2) a meaningless experience may take on a meaning; (3) an experience and its meaning may be disjoined in time; (4) one and the same experience may have several meanings; (5) one and the same meaning may attach to several experiences; (6) meaning and mental process are not co-variants. Examples of each of these cases are given which we do not have space to reproduce here.[34]

In these proofs of the non-meaningful nature of experience, regarded psychologically, it is assumed that a process can take on or lose meaning with no essential change intrinsically, just as the character of a man undergoes no change when he puts on or takes off his overcoat. This assumption, that processes remain constant regardless of their meaningful contexts, still remains to be given the *experimental proof* it needs. It is significant that all of Titchener's examples to prove that meaning is not intrinsic to experience are drawn from every-day life wherein it is impossible to examine the evidence. There is much evidence that 'meaning' does affect process: we have seen how, if kinaesthetic and other secondary processes play a part in the Müller-Lyer illusion, meaning may so permeate the core of sensations that they are more or less modified. Certainly it has not been *proved* that two processes can be identical or similar yet having different meanings. The common experiences of mankind as well as the experiences met with in the laboratory are so shot through with meanings that to assume that these ubiquitous phenomena have no effect upon psychological processes is to overlook one of the most fertile fields requiring study. Certainly this assumption is too big, involves too much, to be swallowed without further test. It should not even be adopted as a working hypothesis for the sake of simplifying problems lest important factors are dismissed which need study. That interesting and important problems have been overlooked by too-ready acceptance of this assumption cannot be denied, although it may have

[34] Beginner's, pp. 26–29.

served a purpose when psychology, in order to be scientific, had to be restricted to simple and uniform stimuli.[35]

CASE 10.—All perceptions under one set of conditions or another may lose their richness of content while apparently 'meaning' as much as ever. In fact, Titchener confesses that "We perceive more than is furnished us by sensations and images. It is a fact that *every perception is shaped and moulded by the action of nerve-forces which show themselves neither in sensation nor in image.*" [36] Behind the characteristic nucleus of sensations and the varying complement of images is 'brain-habit.' The nervous system, he tells us, throws impressions into certain customary forms of 'thing' and 'space.' When, for example, through repetition a percept loses in content, its meaning may still be preserved through the influence of 'brain-habit.'

Brain-habit, then, constitutes the nearest approach to a physiology of perception which we can find in Titchener. Since the brain processes are not described that supposedly underlie perceptual processes nor any hint given as to how he conceived the brain to function, we are left with no tangible physiological theory of perception so far as Titchener is concerned. We are probably right in assuming that his physiology, had he formulated it, would have been as atomistic as his psychology, since summative brain processes might be supposed to parallel summative psychological processes. We need not speculate, however, on what Titchener's fundamental

[35] L. Troland has been practically the only writer of a textbook to see that there is just as much of a problem to account for when the perceptual pattern 'correctly' represents the stimulus-pattern as when this is not the case, for, as he says, "the pattern (even in the normal case) on the cerebral cortex is certainly very dissimilar to that of the corresponding object." He says further: "When we class these relationships as illusions, we can only mean that there is a failure of the forms of the visual projection field patterns to be reasonably similar to those of the object-patterns. However, *there is no reason, a priori, to expect similarity rather than some other type of functional relationship.*" Psychophysiology, vol. 1, p. 335. (Italics mine.) Troland can write thus because he begins with a *functional* relationship between stimulus and perception. His treatment, by the way, is one of the few textbook accounts unencumbered by the clearness-context assumptions and is one of the most refreshing texts dealing with perception the writer has encountered. Another is R. H. Wheeler's The science of psychology, 1929.

[36] Beginner's, p. 115.

assumptions regarding physiological processes were. Brain-habit seems to be no more than a physiological correlate of 'past experience' which we have discussed above and open to the same charges of ambiguity and emptiness. It does not help us to understand why or how the meaning of an object or perceived event can be preserved after the 'context' has dropped away. By what magic an unconscious brain process can carry a conscious meaning (since no context is observable in such cases), we are not told nor can we guess. Having restricted his psychology to a description of observable processes, when these are lacking, nothing is left.

In this examination of the clearness-context theory we have tried to make clear what the theory really means. To do this it has been necessary to take the terms in which it has been expressed seriously and critically. If we pretend to any sort of scientific status whatsoever in psychological discussion we must take words for what they seem to mean. We cannot juggle them arbitrarily to mean one thing at one time and another at another time. This demand may lead to consequences which the author of a theory may deny, especially if he possesses the knowledge of a later day. No person in any day or age can foresee all the implications of what is said at that time for future generations. No individual can be sure that he is even aware of all the motives and assumptions underlying his thinking. It has become the fashion in recent years to state all of the assumptions entering into one's reasoning. While this is desirable it is not always possible. Even if one intends to lay bare the foundations of his thinking there are always present two sets of assumptions: (1) assumptions which are held explicitly and recognized; (2) assumptions which may be present in the very beginning or creep in later but are not recognized until new facts, a different point of view, and the general advance of knowledge force them into the light of discussion. It is highly probable that the author of a theory would not subscribe to some of his assumptions made implicitly. There is no justification, nevertheless, for keeping implicit assumptions under cover for all time, even if their author would hate to admit them, or for denying that they

were ever made.[37] It is the duty of later workers to uncover and face hidden assumptions candidly and without apology. Thus progress is made in science. This we have tried to do with the clearness-context assumptions, knowing full well that a later day and age may discover assumptions and consequences in our thinking which we would not accept, had we the light which guides them.

There is little doubt that Titchener and many of those who re-apply his theory would stoutly refuse to accept either the assumptions or the consequences of certain aspects of the clearness-context theory in the light of present-day knowledge. Two choices remain in such a case if we have rightly interpreted the theory: (1) to introduce other assumptions to meet our criticisms; (2) to begin afresh from the very ground up, making new assumptions to be sure, for thinking cannot go on without them. The first choice is often resorted to surreptiously by claiming that the words used in the statement of a theory or its applications mean something different from what they ordinarily mean. With this type of argument we have little patience and can dismiss it at once. The introduction of bolstering assumptions when new knowledge appears only serves to complicate a theory in the endeavor to make it fit cases it was not designed for. In any case we run the danger of failing to advance with the new in trying to assimilate it to the old. We therefore come to the second alternative, that of starting out afresh. Whether or not one can do this depends upon a number of factors we do not intend to enter upon here. That the clearness-context theory guided research, stimulated thinking and served other useful purposes in the past it is not my intention to deny. No theory that has had its vogue could have failed to do so. But with the apparatus by whose means it was erected, the context

[37] A beautiful example of the attempt to gloss over what the so-called structural school primarily had its eye upon is found in P. C. Squires, A criticism of the configurationist's interpretation of 'structuralism,' *Amer. J. Psychol.*, 1930, 42, 134ff. Squires extracts statements from various writers, including Titchener, which ameliorate the aims and accomplishments of structuralism in the light of *Gestalt* criticism. This note should be read by all who wish as good an apology as could possibly be made for a point of view that finds few defenders today among the younger generation of psychologists.

theory now deserves a niche in the museum where go all pioneer inventions worthy of such honor.

Summary and Conclusions

A critical examination of the statement and applications of the clearness-context theory of perception has revealed the following assumptions which have been challenged:

1. Perceptions can be analyzed by direct inspection;
2. Analysis of perception yields more or less fixed elements in one-to-one correlation with the stimulus-complex;
3. Complexity can be described and explained as an addition of simple units;
4. Besides the psychological elements (sensations and images) perceptions contain meanings which may vary independently of the elements, although meanings arise from the context of elementary processes;
5. Lack of constancy between stimulus and process can be explained through a supposed variation in meaning while process remains constant;
6. Certain processes may be unclear or unnoticed thereby accounting for unexpected deviations from what is normally expected in a given set of stimulus conditions.

We have found that the clearness-context theory fails to square with observed fact when tested, in being open to the following objections:

1. Widely different phenomena are blanketed under vague, ambiguous and general terms like meaning, unclearness, and past experience or its supposed effects;
2. Many phenomena fail to get a positive account, being referred to some preconceived set of cases under which they do not fall;
3. Specific differences in perceptions are neither predicted from it nor accounted for satisfactorily;
4. Assumptions made for special cases fail to account for similar cases which fall in the same class;
5. Changes which we should expect on the basis of the theory fail to appear, and, on the other hand, changes which do appear are not covered by the theory.

Finally, we have found that the theory is internally inconsistent and hence logically untenable.

In the light of these conclusions neither new assumptions nor any re-interpretation of the theory can make it acceptable. Our whole approach to the problem of perception will have to be made from a different angle. While lip-service is being paid to newer doctrines which begin with fresh assumptions, the actual passing of the clearness-context theory has still to be demonstrated by a really changed treatment of perception in the general run of textbooks and articles, and in the prosecution of experimental work.

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A GENERALIZED PSYCHOPHYSICAL LAW

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Some years ago Professor Woodworth [1] proposed a general psychophysical law which seemed to harmonize Weber's law with the Fullerton-Cattell [2] law and to take care of the numerous discrepancies between experimental observations and both laws. The Fullerton-Cattell law was originally stated as follows: "The error of observation tends to increase as the square root of the magnitude, the increase being subject to variation whose amount and cause must be determined for each special case." [3] In symbolic terms, Weber's law may be expressed: $\Delta R_a = KR_a$ and Cattell's law, $\Delta R_a = (KR_a)^{.5}$. ΔR is in both cases some measure of discriminal sensitivity, based upon a prescribed percentage of 'greater' judgments, or in the method of average error it is the probable error of the reproductions of a standard magnitude R_a.

It has long been known that experimental data rarely obey either of these laws exactly, but tend to fit a function, $f(R)$, somewhere between the two. Woodworth proposed a third, more general law, which would include both the others as special cases. It is based upon the same assumptions as Cattell's law, that the difference limen is an average of the errors of observation. A further assumption is made concerning the way in which errors of observation summate when R is increased. In statistical procedure it has been shown that the p.e. of a sum of two variables (X_1 and X_2) is equal to:

$$\text{p.e.}_s = (\text{p.e.}_1^2 + \text{p.e.}_2^2 + 2r_{12}\text{p.e.}_1\text{p.e.}_2)^{1/2}. \qquad (1)$$

In this equation, p.e.$_1$ is the average error of observation of

[1] R. S. Woodworth, Professor Cattell's psychophysical contributions, *Arch. Psychol.*, 1914, **30**, 60–74.

[2] G. S. Fullerton and J. McK. Cattell, Perception of small differences, 1892, 23ff.

[3] G. S. Fullerton and J. McK. Cattell, *op. cit.*, p. 25. Hereafter, in this article, the law will be referred to as Cattell's law, by which name it is usually known.

variable X_1; p.e.$_2$ is the average error of observation of variable X_2, and r_{12} is the Pearson coefficient of correlation between corresponding errors in the two variables. To put the equation into the notation of psychophysics, we have:

$$\Delta(R_1 + R_2) = [(\Delta R_1)^2 + (\Delta R_2)^2 + 2r_{12}(\Delta R_1)(\Delta R_2)]^{1/2}. \quad (2)$$

It can readily be shown that if r_{12} is equal to 1.00, that is, if the variations in the perception of R_1 and R_2 are completely dependent upon each other, Weber's law holds. To take a simple case, let $\Delta R_1 = \Delta R_2$.

Then

$$\Delta(R_1 + R_2) = \Delta(2R_1) = [2(\Delta R_1)^2 + 2(\Delta R_1)^2]^{1/2}$$
$$[4(\Delta R_1)^2]^{1/2} = 2(\Delta R_1).$$

Likewise, if r_{12} is equal to zero, Cattell's law is satisfied for then,

$$\Delta(2R_1) = [2(\Delta R_1)^2]^{1/2} = \sqrt{2}(\Delta R_1).$$

It should be noticed, however, that this is not really a psychophysical law at all, but a psychological one. Both sides of the equation involve psychological quantities. Neither of the dependent variables contains R, but rather ΔR, which is a psychological value, measured in physical units. The law could only have meaning if expressed in the form:

$$\Delta(R_1 + R_2) = f(\Delta R_1, \Delta R_2, r_{12}),$$

since it involves the coefficient of correlation between two variables. It would seem almost impossible, at first thought, to determine this coefficient of correlation empirically. When a subject is judging a line of length $(R_1 + R_2)$, how can we single out the errors in the segment R_1, from those in the segment R_2 to see whether or not they are dependent or independent? And does this r_{12} have any real methodological significance?

It is possible to determine the value of r_{12} indirectly, however, if the three other variables in equation (2) above are known, that is variables (ΔR_1), (ΔR_2), and $\Delta(R_1 + R_2)$, for by using equation (2), one can solve for r_{12} as follows:

$$[\Delta(R_1 + R_2)]^2 = (\Delta R_1)^2 + (\Delta R_2)^2 + 2r_{12}(\Delta R_1)(\Delta R_2).$$

Transposing:

$$2r_{12}(\Delta R_1)(\Delta R_2) = [\Delta(R_1 + R_2)]^2 - [(\Delta R_1)^2 + (\Delta R_2)^2],$$

and

$$r_{12} = \frac{[\Delta(R_1 + R_2)]^2 - [(\Delta R_1)^2 + (\Delta R_2)^2]}{2(\Delta R_1)(\Delta R_2)}. \qquad (3)$$

Some recently published data of Robinson and Robinson,[4] seem to contain just what one would want in order to put this law to an empirical test. In the study of these authors, 81 subjects were asked to reproduce 40 lines of ten different lengths. The shortest length was 60 mm. and the longest 330 mm. They varied in steps of 30 mm. The average length of line reproduced by each subject for each standard is given in a convenient table.[5] It is possible, therefore, not only to find the average error of observation for each length of line reproduced, but also to correlate the single observations for each length with those of every other length. It is assumed, of course, that the mean length of 40 reproductions of any standard length by one subject constitutes a single observation. This is, perhaps, an unusual method of measuring difference limens, but it seems justifiable, and it is the only feasible way that a good measure of the correlation between observations of segments of a length can be obtained empirically. Having determined, for example, the standard deviation of the distribution of mean judgments on a line of 90 mm., and likewise on a line of 120 mm., and having correlated the 81 individual mean judgments of these two lengths, one can apply Woodworth's formula (formula (2) above), and find the theoretical standard deviation of the distribution of judgments for a line of 210 mm. If this checks with the actual S.D. of the observations on a line of 210 mm. by these same 81 subjects, and if this holds true for a large number of similar judgments, one might put considerable credence in the formula. If the discrepancies between theory and fact are too great to be explained by chance, something is wrong with the law, and perhaps with the assumptions upon which it is based.

[4] E. S. Robinson and F. R. Robinson, A simple series of abilities, *Amer. J. Psychol.*, 1929, 41, 33–53.

[5] E. S. Robinson and F. R. Robinson, *op. cit.*, p. 37ff.

Using the Robinson-Robinson data, standard deviations were found for the 81 observations on each length of line. But it was noticed that one subject gave judgments consistently very much lower than the remainder of the group. For some lengths his value was more than five times the S.D. His judgments, therefore, tended to lower the group means and to raise the S.D.'s so unduly that his judgments were entirely eliminated.[6] The means and S.D.'s of the data from the remaining 80 subjects are given in Table 1.

TABLE 1

MEANS AND STANDARD DEVIATIONS OF THE REPRODUCTIONS OF LINES

Length of Standard in Millimeters	Mean of 80 Judgments (R)	Standard Deviation (ΔR)
60	75.0	8.98
90	107.4	9.85
120	138.9	10.56
150	168.7	11.36
180	194.6	12.59
210	230.1	14.07
240	260.4	15.37
270	291.3	16.61
300	321.1	17.50
330	347.5	17.66

It will be noticed that the lines were all overestimated. The means of the judgments will be used as the R-values in what is to follow, rather than the standard lengths, because it is about these lengths, the means, that the errors of observation really occur and not about the standards themselves.

The coefficients of correlation between the different variables are given in Table 2. These differ slightly from those given in Robinson and Robinson's article [7] due to the elimina-

TABLE 2

INTERCORRELATIONS BETWEEN JUDGMENTS FOR VARYING LENGTHS OF LINES

	60	90	120	150	180	210	240	270
60	.956	.880	.929	.925	.481	.601	.423	.348
90		.962	.963	.833	.700	.650	.489	
120			.932	.896	.786	.796		
150				.952	.933			

[6] This was subject number 24, p. 37.

[7] E. S. Robinson and F. R. Robinson, *op. cit.*, p. 38.

tion of the anomalous subject, but they follow the same general law which was found there: the greater the difference between the standard lengths used, the lower is the correlation between them. This rather wide variation in the correlation of observations of different lengths on the surface looks unfavorable for Woodworth's law. The latter law might be expected to demand a uniform correlation between such observations within the same kind of stimuli. That is, Weber's law, or Cattell's law or any other law between them might be expected to hold within the same range of stimuli. Only these coefficients that were needed in this study are given. The coefficients for self-correlations are taken directly from Table II of Robinson and Robinson, since the writer had no means of finding self-correlations from the data presented in the article. The other coefficients differ so little from those given by the authors, that it is probable that errors in the self-correlations used in this study are negligible.

It was possible, next, to apply Woodworth's formula, predicting the S.D.'s of the longer lines (120 to 330 mm.) from the combinations of shorter lines (60 to 270 mm.). These results are presented in Table 3. The discrepancies between

TABLE 3

LARGER S.D.'S PREDICTED FROM SMALLER S.D.'S BY USING WOODWORTH'S FORMULA

Predicted from $R_1(+R_2)$ / R_1 R_2	Standard Lengths of Lines							
	120	150	180	210	240	270	300	330
60	17.76	18.26	19.19	19.96	18.65	20.75	20.82	21.46
90			19.51	20.22	20.31	20.72	21.80	21.94
120					21.34	21.34	21.89	23.39
150							22.44	23.55
Observed S.D.'s	10.56	11.36	12.59	4.07	15.37	16.61	17.50	17.66

predicted and observed S.D.'s are all positive and very large. They range from 3.28 to 7.20, with a mean of 5.14. In this case, at least, theory does not fit fact very well.

It will be noticed in Table 3 that for any one length, *e.g.*, the line 300, the predicted S.D.'s increase as the two components approach equality. It is to be remembered that as

the two segments of the longer lines approach equality, the coefficients of correlation, r_{12}, approach 1.00. This, coupled with the fact that the predictions are all much too large, should indicate that r_{12} is a superfluous member of Woodworth's formula.

We can see even more strikingly the wide gap between theory and fact, if we reverse the procedure and solve for r_{12} from the other three variables, using formula (3). Table 4 shows these results. Eleven of the twenty coefficients are

TABLE 4

PREDICTED COEFFICIENTS OF CORRELATION BETWEEN ERRORS OF JUDGMENT IN SEGMENTS OF A LINE

	Standard Length of Line							
	60	90	120	150	180	210	240	270
60	−.309	−.275	−.177	−.075	−.008	−.011	−.039	−.150
90		−.183	−.051	.045	.082	.041	−.071	
120			.059	.147	.136	.008		
150				.187	.085			

negative. The total range of the predicted coefficients is from −.309 to +.187. The actual range, to be found in Table 2, is quite different, being from .348 to .962. Such a wide divergence could not be due to chance. It is possible, of course, that Woodworth had another type of correlation in mind. If so, the predicted coefficients indicate a psychophysical function nearer to Cattell's law (r_{12} equals zero) than to Weber's (r_{12} equals 1.00), in fact, even beyond Cattell's law in the cases of negative coefficients.

From the obvious failure of Woodworth's law to apply to these data, the writer suspected that r_{12} is not the necessary factor in adjusting data to the correct psychophysical law. It seemed necessary to scrutinize more closely the bare relation between ΔR_a and R. Accordingly, the ΔR_a values were plotted against the R values, as in Fig. 1. A straight line might easily be fitted to the ten observed points, but such a best-fitting straight line would never pass through the origin. This condition must obtain, theoretically, for any psycho-

physical law, for when R_a is zero, ΔR_a must also be zero. Of course, we are dealing here in theory with a case that can never be experienced; with an R value even below the *RL*. The most obvious type of function that will pass through the origin is a power function. An attempt to fit a logarithmic function to the data met with imperfect success. A function of the form:

$$\Delta R_a = K R_a^n, \tag{4}$$

was found to be very satisfactory. The two constants, K and n, were found by the method of least squares, giving a formula: $\Delta R = .576 R^{.588}$, or not far from Cattell's law. This function has been drawn through the points in Fig. 1. It will

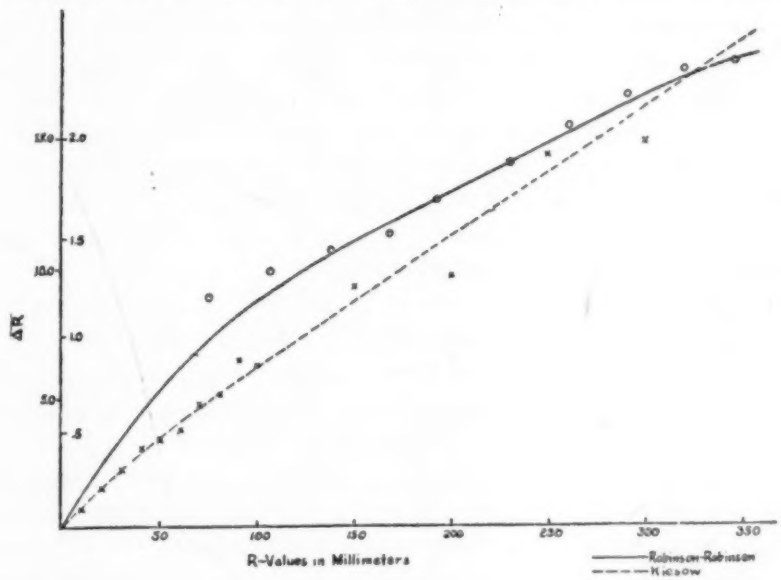

FIG. 1.

be noticed that the first two points, for R equals 75 and R equals 107, deviate noticeably from the curve. These deviations might be serious arguments against a power function but for the stubborn theoretical demand that the curve shall pass through the origin. These discrepancies, as well as the discrepancy for the last point at the other end of the curve, might be explained on the basis of an 'end effect.' It may be

that the limited series of lengths used as standard stimuli produce a kind of closed system, and that variations in judgment are influenced by this system, and that the influence is greatest at the ends of the series. Similar influences have been observed in some unpublished work of the writer involving a series of lifted weights. In spite of these deviations of a few points from the curve, the correlation between observed points and corresponding points on the curve amounted to .970, after correction was made in the coefficient for the number of constants involved.[8]

As a check upon this type of law, other data were sought. In a recent study of Kiesow,[9] some additional data on judgments of lines were found. The standard lines ranged from 10 to 300 mm. in length. This enables us to test the law in a region nearer the origin. The judgments were obtained by the method of minimal changes from a single subject who made a hundred observations upon each standard. The limens of this study were secured in the more orthodox psychophysical manner, so that any objections raised to the use of the Robinson-Robinson data because the limens were obtained in an unusual fashion, will be adequately met.

From Kiesow's own computations, it is obvious that Weber's law was not fulfilled. The Weber ratios were far from constant; they ranged from 1/95 to 1/153. Assuming a power function, and fitting by the method of least squares, the following formula was found to hold: $\Delta R = .0157R^{.864}$. In this case the observed points coincide with the curve even to the smallest values of R (see Fig. 1). The correlation between observations and theoretical values was .998. No end effects have to be explained here. This may be due to the fact that the method of minimal changes was used instead of the method of average error, as in the Robinson-Robinson experiments. With a value for n as large as .864, one would conclude that the results approach Weber's law more closely than they do Cattell's.

[8] M. Ezekiel, Methods of correlation analysis, 1930, p. 120.

[9] E. Kiesow, Über die Vergleichung linearer Strecken und ihre Beziehung zum Weberschen Gesetze, *Arch. f. d. ges. Psychol.*, 1926, **56**, 421–462.

It is interesting, now, to put the power law in the form of Woodworth's equation, but with the expression containing r_{12} missing. This can be done with only a few transformations: From (4),

$$\Delta(R_1 + R_2) = K(R_1 + R_2)^n, \tag{5}$$

$$R_1{}^n = \frac{\Delta R_1}{K},$$

and likewise,

$$R_2{}^n = \frac{\Delta R_2}{K},$$

so that

$$R_1 = \left(\frac{\Delta R_1}{K}\right)^{1/n},$$

and

$$R_2 = \left(\frac{\Delta R_2}{K}\right)^{1/n}, \tag{6}$$

Substituting (6) in (5),

$$\begin{aligned}\Delta(R_1 + R_2) &= K\left[\left(\frac{\Delta R_1}{K}\right)^{1/n} + \left(\frac{\Delta R_2}{K}\right)^{1/n}\right]^n \\ &= K\left[\frac{1}{K^{1/n}}\left\{(\Delta R_1)^{1/n} + (\Delta R_2)^{1/n}\right\}\right]^n \\ &= \left[(\Delta R_1)^{1/n} + (\Delta R_2)^{1/n}\right]^n \end{aligned} \tag{7}$$

The similarity of formula (7) to Woodworth's (2) is obvious, especially if n equals $\frac{1}{2}$. Only the expression including r_{12} is lacking, and in formula (7) the power coefficient is left flexible to fit the special data to be considered. It is maintained that this flexibility in the power coefficient will do what Woodworth had hoped to do by introducing his error formula for the probable error of a sum of variables as in formula (1). Both Weber's and Cattell's laws are simplified and special cases of the more general power function. The coefficients K and n, especially n, when determined for any set of data, may have very significant meanings when they are compared with the same coefficients from other data. In the same manner different sense fields can be compared, and results from different perceptual stimuli, such as lines, time intervals and tonal patterns, may be brought into line, and general

quantitative laws derived. Even data from the field of learning and memory might be compared in this way with psychophysical data, and more general laws of mind established. The same coefficients might be found in physiological data that were obtained under known conditions having a bearing upon the psychological functions just mentioned, and physiological theories might thus be advanced to account for them.

The power function also takes the psychophysical law out of the realm of measurement of 'errors of observation' and puts it in a more rational form. Woodworth's law assumes that (1) we are measuring errors of observation, and (2) that errors summate according to the laws of chance, except as errors in different segments of a stimulus are correlated. If r_{12} in his formula is zero, the summation is a pure chance affair. If r_{12} is other than zero, some dependence exists between sub-errors in an observed quantity. In the above discussion of the Robinson-Robinson data, the values for r_{12} have to be slightly negative in some cases and positive in others. It is almost inconceivable that such negative relationships should exist, and that the coefficients can be now negative and now positive in such a homogeneous set of observations. And if we grant that the correlations found by Robinson and Robinson are of the type demanded by Woodworth's formula, certainly there is a wide discrepancy between the actual and theoretical coefficients.

It might be maintained that r_{12} tells us how much departure from pure chance we have in any psychophysical measurements. And it is departures from chance that we are interested in, for chance is another name for ignorance, and departures from chance mean knowledge. It is possible that the power formula is even more significant in this respect. Pure chance relationships exist between two sets of judgments, according to Woodworth, when r_{12} is zero and when n of the general power formula, is equal to .5. Variations in n above and below the value of .5, therefore, may tell us all that the variations in r_{12} were supposed to reveal in Woodworth's formula, and perhaps a great deal more. Furthermore, the simple power formula obviates the practice of finding the value of r_{12}, which is not an easy task.

Let us see how well the simple power formula, in the form analogous to Woodworth's formula, that is in the form of formula (7), satisfies the Robinson-Robinson data. This formula, with the particular constants that are required, becomes:

$$\Delta(R_1 + R_2) = [(\Delta R_1)^{1.7} + (\Delta R_2)^{1.7}]^{.588}.$$

The ΔR's of the longer lines (standards 120 to 330) were predicted from those of the shorter lines, just as in Table 3. The results are given in Table 5. The discrepancies are again mostly positive, but not nearly as great as those in

TABLE 5.

LARGER S.D.'S PREDICTED FROM SMALLER S.D.'S BY USING THE POWER FORMULA

Predicted from R_1	Standard Lengths of Lines							
	120	150	180	210	240	270	300	330
60	13.48	14.15	14.71	15.35	16.35	17.33	18.72	19.80
90			14.79	15.33	15.96	16.94	17.09	19.25
120					15.86	16.47	17.43	18.36
150							17.06	18.00
Observed S.D.'s	10.56	11.36	12.59	14.07	15.37	16.61	17.50	17.66

Table 3. The range of the discrepancies is from $-.44$ to $+2.92$ with an absolute mean of 1.14. The tendency to positive discrepancies can be attributed almost entirely to the abnormally high values for the two ΔRs for standards 60 and 90 which figure in fourteen of the twenty predictions. Aside from these fourteen, the differences are small and both positive and negative. It is safe to assert in view of the proof of formula (7), that if all points fit the function $\Delta R = KR^n$, predictions by formula (7) would be perfect, and no other factor, like r_{12} is needed in the law of the combination of errors.

Just what the consequences are for Cattell's first assumption, that the *DL* measures errors of observation, is hard to say. The bridge between power functions such as has been proposed and error functions, may not be hard to cross, but the writer will not attempt to do so here.

Further survey of the literature for data bearing on Weber's law will be needed to establish the generality of the

power law which has been suggested here. One very convenient set of data recently published will now be examined as a further test of the power law in another sense field. The data are for threshold measurements of intensity of sounds in a study of Kenneth and Thouless.[10] Incidentally, the authors tried to show that there is a continuity between the *RL* and *DL* for intensities of sound. One set of their measurements, which is typical of all their results, will be seen in graphic form in Fig. 2. A power function has been fitted to

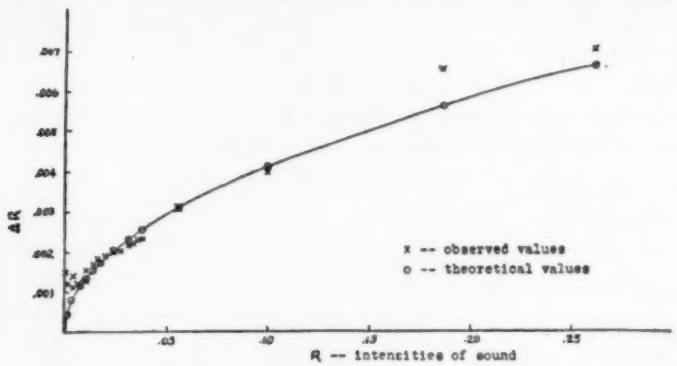

FIG. 2.

these points, with the exception of the first five points which are in the region of the *RL*. In fact, the very first point is the measure of the *RL*. Aside from these five points, a function with the formula: $\Delta R = .01276R^{.489}$, fits the points very well. The coefficient of correlation, corrected, is .990.

The glaring discrepancies for the first few points, near the *RL* mean either that the power function is wrong, or that Kenneth and Thouless' conclusion is wrong. The authors themselves point out that the Weber ratio is not constant; Weber's law does not hold. It has universally been admitted that Weber's law does not hold for small values of *R* and for very large values of *R*. The power law seems to take care of small values of *R*, even down to the neighborhood of the *RL*. It is easy to see how Weber's law would seem to hold for medium values of *R*, for in that region the power function

[10] J. H. Kenneth and R. H. Thouless, Relationship between the absolute and differential thresholds for an auditory stimulus, *Amer. J. Psychol.*, 1930, 42, 389–398.

could approach linearity for relatively short distances. It still remains to find out what happens to the function with very high and with very low values of R. If there is a significant change in the direction of the curve for very high values of R as well as for very low values, as in the Kenneth and Thouless data, then three possibilities remain: (1) A reformulation of the power law will be necessary to take care of the end values; (2) Near the RL and TR, other functions overlap the power function; (3) Continuity breaks down entirely, and no functions hold for the end values. These end regions will need more careful inspection; the work of Kenneth and Thouless is a step in the right direction. In the meantime, it will be profitable to attempt to apply the power assumption to data in all fields of perception, as the writer has only begun to do in this study. There is some assurance that this assumption will take care of a very wide range of phenomena.

Summary

Woodworth has previously suggested a generalized psychophysical law, based upon the assumption that in psychophysics we are measuring errors of observation, and that in observing larger magnitudes the errors of observation summate according to the laws of chance. It was shown that Weber's law, and Cattell's substitute law were special cases of Woodworth's general law. The writer applied Woodworth's formula to data on the judgment of lengths of lines and showed that it did not hold. It was proposed that these psychophysical laws be recast in the form of a general power function: $\Delta R_a = KR_a^n$, in which K and n are experimental constants. In Weber's law, n equals 1; in Cattell's law, n equals .5. Data from the judgment of lines and sound intensities were adjusted to power functions with success.

[MS. received June 8, 1931]

DISCUSSION

A NEW METHOD FOR THE MEASUREMENT OF WILL-POWER: A PRELIMINARY REPORT

There exist two principal methods which purport to measure the dynamic part of will-power quantitatively, as contrasted with the numerous reaction-tests which investigate the will-phenomena in their relation to stimulation. The so-called combined method originated in 1905 by N. Ach (1) is based on the idea that the effect of volition depends on one side upon the intensity of the determination and on the other upon the hindrances opposed to its realization. Therefore we are able to measure the dynamic part of volition by raising internal hindrances in the form of opposed habits. Ach used artificially created associations between nonsense syllables and altered the intensity of the association by varying the number of repetitions. The most significant result of his experiments was the fact that no amount of repetition was able to overcome the determination, it becoming evident therefore that the hindrances raised were not sufficiently strong to be used as measure.

We are confronted therefore with the problem of finding a habit sufficiently deep-rooted to serve as a measure of volition. J. E. Downey (2) thought to have found this habit in the forms of writing and other unconscious muscular movements. The number of times the involuntary impulses of the subject cause him to leave the line which he is asked to trace, is taken as a measure of his conscious will-effort. The idea is ingenious and the only objection to be made is the large part which sense-impressions and muscular coördination play in it. In accordance with this, the reliability of the different Downey-tests varies greatly, self-correlations ranging from $+.12 \pm .06$ to $+.89 \pm .01$, as has been ascertained by R. L. Uhrbrock (3).

Based on these considerations, the writer has sought to devise a test based on a deep-rooted habit, for which the muscular and nervous training are equal to all subjects. This habit has apparently been found in unconscious respiration, for the reason that there is hardly a habit more deep-rooted and general. In fact, conscious voluntary respiration is exceptional. All persons are equally well trained in breathing and so there is no necessity to take into consideration nerve-sensitivity, muscular coördination and exhaustion.

The technic of the experiment devised may be described as follows: The subject is placed in a comfortable position, and a pneumograph is affixed to his chest which may be advantageously combined with a self-registering apparatus. The pneumograph's pressure on the chest should be as light as possible, in order to prevent any muscular exhaustion. The subject is then required to breathe voluntarily for as long a time as possible and to indicate his volitional impulse by making each voluntary inspiration a little deeper than his unconscious breathing, in order that the difference may be registerable. As soon as the subject feels tired, the experiment should be interrupted. The number of voluntary impulses that the subject is able to give in spite of his habit of unconscious breathing is taken as index of the intensity of his determination and his will-concentration.

With this device preliminary experiments have been undertaken which have given promising results inasmuch as the individual differences determined have been in most cases significant. Further experiments are under way to investigate the individual, sexual and racial variability of will-power and its mode of inheritance.

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ON THE LIMITS OF HUMAN VARIABILITY

In a paper delivered at the Ninth Congress of International Psychology,[1] I called attention to the fact that the range of a great many human capacities, when defined in terms of the number of times the uppermost measure is that of the lowest, approaches the ratio of 2 : 1. Further investigation has shown, on the one hand, that for a preponderant number the ratio is more nearly 2.5 : 1, and on the other hand, that for certain types of anthropometric traits, namely those of linear dimensions such as heights, it is considerably

[1] Printed in full in *Scient. Mo.*, 1930, 31, 35–39. More extensive discussion of the entire problem of the Range of Human Capacities, and the data upon which the generalizations are based will appear in a book of the same title now in preparation.

less, that is, about 1.3 : 1. Also, that in the case of some metabolic processes the ratio may be further reduced, so that, to take the most singular example, the case of normal body temperature, it may be as low as 1.03 : 1.

It appears that the 'simpler' the processes or capacities measured the smaller the variability, that is the ratio between the extremes, the term *simple* applying primarily to the method or conditions under which the measurements are obtained rather than to the intrinsic character of the trait or capacity considered. Thus height is classed as a simpler trait than weight, and oxygen consumption per minute as simpler than general intelligence; but it is obvious that even those traits which we may designate the simplest are in reality already very complex. Take, as an example, stature. For the purpose of practical classification we may call it or even assume it to be simple as compared to such a trait as vital capacity or general intelligence, but in point of fact we know it to be the resultant of an indefinitely large number of factors whose compounded cause or effect we term growth. And so on of any other human trait or capacity which we might choose. Now, to obtain the lowest limits of variability we should have to hit upon a trait which is determined by only a single factor. Such an ideal case, however, is non-existent, and for this reason the calculation of the lower limits of human variability is not possible. But it can readily be shown, as I propose presently to do, what inevitably are the true upper limits of variability of any trait or function, however complex its character, that is, upon however many factors it may depend.

I start with the assumption which experience amply justifies, namely, that all human traits are complex variables dependent upon a large number of factors. Of the variability of these ultimate or 'single' factors we know nothing other than that taken individually they must be small, since when already compounded they give small values (*e.g.*, height 1.26 : 1, body temperature, 1.03 : 1). Now let T be any trait which in our present argument is dependent upon an indefinitely large number of factors, $a, b, c, d, e, \ldots n$. Then as,

$$T \propto a, b, c, d, e, \ldots n$$
$$T = a \times b \times c \times d \times e, \ldots \times n.$$

Calling V_T the variability [2] of T, then, if the function is continuous, as all measurements of human traits in fact are, it can be shown

[2] Where the variability, V, is defined as the ratio between the extremes; for practical ways of calculating this, see paper already cited.

mathematically that

$$V_T = V_a \times V_b \times V_d \times V_d \ldots \times V_n, \qquad (1)$$

where

$V_a, V_b, V_c, \ldots V_n$ are the corresponding variabilities of the individual factors $a, b, c, d, \ldots n$.

The variability we are here concerned with is the total range for whose measurement we take the ratio between the extreme or limiting values. Call T_0 the lower limit of the trait; then the upper limit, T_u, may be defined as $T_0 + dT$, where dT is a difference less in magnitude than T_0 (as actual data shows it to be). Then by definition

$$V_T = \frac{T_u}{T_0} = \frac{T_0 + dT}{T_0},$$

which may be rewritten,

$$V_T = 1 + \frac{dT}{T_0}. \qquad (2)$$

In the same way, $V_a, V_b, V_c, \ldots V_n$, may be written as

$$V_a = 1 + \frac{da}{a_0}$$

$$V_b = 1 + \frac{db}{b_0}$$

$$V_c = 1 + \frac{dc}{c_0}$$

$$\ldots\ldots\ldots\ldots$$

$$\ldots\ldots\ldots\ldots$$

$$V_n = 1 + \frac{dn}{n_0}.$$

Substituting in (1)

$$V_T = \left(1 + \frac{da}{a_0}\right)\left(1 + \frac{db}{b_0}\right)\left(1 + \frac{dc}{c_0}\right) \cdots \text{to } n \text{ factors.} \qquad (3)$$

Whatever the value of $V_T, V_a, V_b, V_c, \ldots$ must necessarily be smaller; also as we increase the number of factors, $a, b, c, \ldots$, the values of $da/a_0, db/b_0, dc/c_0 \ldots$ will progressively diminish and the differences between them even more so, so that if we take n indefinitely large, the differences will become negligible, that is,

$$\frac{da}{a_0} - \frac{db}{b_0}, \quad \frac{da}{a_0} - \frac{dc}{c_0}, \quad \frac{db}{b_0} - \frac{dc}{c_0}, \ldots, \text{ etc.,}$$

will all approach zero as a limit. Whence by equating them, we get

$$\frac{da}{a_0} = \frac{db}{b_0} = \frac{dc}{c_0} \cdots = k.$$

We do not know the value of k, other than that it is small and must be less than 1. Let $k = 1/x$; then by substituting in (3) we get,

$$V_T = \left(1 + \frac{1}{x}\right)\left(1 + \frac{1}{x}\right)\left(1 + \frac{1}{x}\right) \cdots \text{ to } n \text{ factors, or}$$

$$V_T = \left(1 + \frac{1}{x}\right)^n \cdots. \tag{4}$$

The value of the term in the parenthesis depends on the relative values of x and n. We do not know what is the value of $1/x$ but do know that as n increases $1/x$ becomes decreasingly small, or what amounts to the same thing, x becomes increasingly large. As n approaches infinity x will also approach infinity, and hence by taking n sufficiently large, we may substitute n for x, and (4) becomes

$$V_T = \left(1 + \frac{1}{n}\right)^n = 2.7182 \cdots, \tag{5}$$

that is, our old friend epsilon (ϵ) or the limiting value of organic rate of growth that seems characteristic of all biologic phenomena.

It thus appears that human variability is limited biologically by the law of organic rate of growth and that therefore under normal conditions we may not expect the total range ratio to exceed the amount of 2.718 : 1. The fact is that measurement of a large number of traits and abilities shows this to be true in most cases. There are some exceptions, but in my opinion these are due to the uncertainties of our units of measurement and to some special factors the discussion of which, however, is beyond the intent of this paper.

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